THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

THE K. Keith presenty

THE EDINBURGH NEW PHILOSOPHICAL JOURNAL,

EXHIBITING A VIEW OF THE

PROGRESSIVE DISCOVERIES AND IMPROVEMENTS

IN THE

SCIENCES AND THE ARTS.

EDITORS.

THOMAS ANDERSON, M.D., F.R.S.E., &c.,
REGIUS PROFESSOR OF CHEMISTRY IN THE UNIVERSITY OF GLASGOW;
SIR WILLIAM JARDINE, BART., F.R.S.E., &c.,
AND
JOHN HUTTON BALFOUR, M.D., F.R.S.E., &c.,
PROFESSOR OF MEDICINE AND BOTANY IN THE UNIVERSITY OF EDINBURGH.

JANUARY ..... APRIL 1855.

VOL. I. NEW SERIES.

EDINBURGH:
ADAM AND CHARLES BLACK.
LONGMAN, BROWN, GREEN, & LONGMANS, LONDON.
MDCCCLV.

EDINBURGH:
PRINTED BY NEILL AND COMPANY, OLD FISHMARKET.

CONTENTS.

SCIENTIFIC INTELLIGENCE:—

ZOOLOGY.

GEOLOGY.

CHEMISTRY.

BOTANY.

MINERALOGY.

CONTENTS.

REVIEWS:—

CORRESPONDENCE:—

PROCEEDINGS OF SOCIETIES:—

SCIENTIFIC INTELLIGENCE:—

ZOOLOGY.

GEOLOGY.

ERRATA IN LAST NUMBER.

Page 146, line 37, *for* geology, *read* zoology,
" 149, line 3, *for* layworm, *read* lugworm,
" 153, line 32, *read* "the services that we find the scientific. Amidst arduous duties a naval officer," &c.
" 154, line 16, *for* Now, read That
" 155, line 10, *read* "It is true that natural history, unlike its sister sciences, physics and chemistry," &c.
" 168, line 33, after the word birds, *insert* are regarded,

THE
EDINBURGH NEW
PHILOSOPHICAL JOURNAL.

On the Means of realizing the Advantages of the Air-Engine.

By William John Macquorn Rankine, Civil Engineer, F.R.SS. Lond. and Edin., &c.*

Section I. Summary of the Laws of the Mutual Relations of Heat and Mechanical Power, and of the Theoretical Efficiency of Thermo-Dynamic Engines.

1. The principal object of this paper is to explain the advantages of certain improvements in air-engines, and the reasons for believing that, with these improvements, such engines will be found to be the most economical means of developing motive power by the agency of heat. For this purpose, it is necessary, in the first place, to state briefly the general principles which have been established by the joint agency of reasoning and experiment respecting the mutual relations of heat and motive power, and which are applicable to steam, air, and all substances whatsoever.

It is a matter of ordinary observation, that heat, by expanding bodies, is a source of mechanical power; and conversely, that mechanical power, being expended either in compressing bodies, or in producing friction, is a source of heat.

---

* Read to the British Association for the Advancement of Science, Section G, at Liverpool, September 1854.

The general rules according to which these phenomena take place, have for some time been determined empirically, with more or less precision, for certain particular substances—for example, for steam; but all systematic knowledge respecting them, as they affect all substances whatsoever, is deducible from two laws—that of the mutual convertibility of heat and mechanical power, and that of the efficiency of thermo-dynamic engines; the term *thermo-dynamic engine* being used to denote any body, or assemblage of bodies, which produces mechanical power from heat.

2. THE LAW OF THE MUTUAL CONVERTIBILITY OF HEAT AND MECHANICAL POWER is this: *That when mechanical power is produced by the expenditure of heat, a quantity of heat disappears bearing a fixed proportion to the power produced; and conversely, that when heat is produced by the expenditure of mechanical power, the quantity of heat produced bears a fixed proportion to the power expended.*

This law was believed, and reasoned from, by some inquirers before it was proved by experiment; but being inconsistent with the formerly prevalent hypothesis of the existence of a peculiar substance as the cause of the phenomena of heat, it was recognised by few, until Mr Joule, by experiments on the production of heat by the friction of the particles of various substances, solid, liquid, and gaseous, not only demonstrated the mutual convertibility of heat and mechanical power, but ascertained the fixed proportion which they bear to each other in cases of mutual conversion, which is this: The *unit of heat* generally employed in Britain—that is to say, so much heat as is sufficient to raise the temperature of *one pound of water*, at ordinary temperatures, by *one degree of Fahrenheit's thermometer*—requires for its production, and produces by its disappearance, in other words, is *equivalent to*, 772 *foot-pounds of mechanical power*; that is to say, so much mechanical power as is sufficient to lift a weight of one pound to a height of 772 feet.*

---

* The value of Joule's equivalent for a degree of the centigrade scale is $772 \times \frac{9}{5} = 1389{\cdot}6$ foot-pounds. For French measures, viz., centigrade degrees, kilogrammes, and metres, it is 423·54 kilogrammètres. See Philosophical Transactions, 1850.

This quantity is known by the name of *Joule's equivalent*, and may be otherwise termed the *dynamical specific heat of liquid water* at ordinary atmospheric temperatures.

3. *Illustrations of this law.*—The dynamical specific heats of other substances may be determined either by direct experiment, or by ascertaining their ratios to that of water. For example, to heat one pound of atmospheric air, maintained at a constant volume, by one degree of Fahrenheit, requires the expenditure of

130·5 foot-pounds of mechanical power.*

This is the *real dynamical specific heat* of air. The *apparent dynamical specific heat* of one pound of air, *under constant pressure*, is (for a degree of Fahrenheit)

183·7 foot-pounds;*

the difference, or 53·2 foot-pounds, being the mechanical power exerted by the air in expanding, so as to preserve the same pressure, notwithstanding the increase of its temperature by one degree. The apparent specific heat of air at constant pressure exceeds the real specific heat in the ratio of 1·41 : 1.

All quantities of heat may be thus expressed by equivalent quantities of mechanical power. The heat required to raise one pound of liquid water from the freezing to the boiling point, and to evaporate it at the latter temperature, is

$$1147^\circ{\cdot}5 \times 772 = 885{,}870 \text{ foot-pounds},$$

of which $180^\circ{\cdot}0 \times 772 = 138{,}960$ foot-pounds is what is termed *sensible heat*, or the heat employed in raising the temperature of the water, while the remainder

$$967^\circ{\cdot}5 \times 772 = 746{,}910 \text{ foot-pounds}$$

is the *latent heat of evaporation* of one pound of water at 212° Fahr., being the heat which disappears in overcoming the mutual attraction of the particles of water, and the external pressure under which it evaporates.

The mechanical equivalent of the available heat produced

---

* It is worthy of remark, that the values of the specific heats of air were *predicted*, to a close approximation, by means of the Mechanical Theory of Heat, three years before they were ascertained by M. Regnault's experiments. (Trans. Roy. Soc. Edin., vol. xx.; Comptes Rendus, 1853.)

by *one pound* of such kinds of coal as are commonly used for engines in Britain may be taken on an average as equal to that of the heat required to raise *seven pounds of water* from the temperature of 50° to 212° Fahr., and to evaporate it at the latter temperature; that is to say, in round numbers,

6,000,000 foot-pounds.

The total heat produced by the combustion of the coal is considerably greater; but a portion necessarily escapes with the gases which ascend the chimney, and the above may be considered as a fair average estimate of the mechanical equivalent of that which is practically available.

4. *Mechanical Hypothesis respecting Heat.*—Heat, being convertible with mechanical power, is convertible also with the *vis-viva* of a body in motion. The British unit of heat, one degree of Fahrenheit in a pound of liquid water, is equivalent to the *vis-viva* of a mass weighing one pound, moving with the velocity of 223 feet per second, being the velocity acquired in falling through a height of 772 feet. A mass of water, of which each particle is in motion with this velocity, has its temperature elevated by one degree of Fahrenheit, upon the extinction of the motion, by the mutual friction of the particles.

It is natural to suppose that the motion, during this phenomenon, has not been really destroyed, but has been converted into revolutions of the particles in vortices or eddies too small to be perceptible by any of our modes of observation; and that the centrifugal force of such eddies is the cause of the tendency of hot bodies to expand, melt, and evaporate.

A hypothesis of this kind has long been entertained, and within the last few years it has been used so as to deduce the laws of the mechanical action of heat from the principles of ordinary mechanics, to a certain extent in anticipation of the results of experiment.* As those laws, however, have now been exactly ascertained by experiment, it must be borne in mind, that their certainty is in no way dependent on the truth of the hypothesis in question; the probability of the hypothesis being, on the contrary, dependent on the truth of the laws.

---

* See Transactions of the Royal Society of Edinburgh, vol. xx.

5. *Threefold Effect of Heat.*—The communication of heat to a substance produces, in general, three kinds of effects (setting aside chemical, electrical, and magnetic phenomena, as being foreign to the subject of the present paper):—

1*st*, An increase of temperature and expansive pressure; that is to say, an increased tendency to the communication of heat to other bodies, and to the development of mechanical power by expansion.

2dly, A change of volume; which, under a constant pressure, is an *increase* for every substance, except some liquids near their freezing points.

3dly, A change of molecular condition; as from the solid to the liquid state, or from the liquid or solid to the gaseous state, or any imperceptible change of molecular arrangement; the change to the gaseous state being always accompanied by an increase of volume.

The heat which produces the first of those effects is known by the name of *sensible heat*, as retaining the form of heat, and, in short, *making the body hotter*.

The heat which produces the second and third of those effects is called *latent heat*, as having disappeared in developing a mechanical effect, and being capable of reproduction by reversing the change which caused it to disappear.

Changes of volume are in general accompanied by changes of molecular arrangement or condition, perceptible or imperceptible. The *latent heat of expansion*, or of *evaporation*, therefore, as the case may be, consists partly of heat which disappears in overcoming external pressure, and partly of that which disappears in overcoming the mutual attraction of the particles of the body.

The latter forms by far the greater part of the *latent heat of evaporation*. For example, as already stated, there disappears, in evaporating one pound of water at 212°, a quantity of heat equivalent to

746,910 foot-pounds.

The pressure of the steam produced is 2116·4 lb. on the square foot. Its volume is not known exactly by experiment, but is probably about 26½ cubic feet more than that of the liquid water. Multiplying these two quantities together, it appears that the heat expended in overcoming external pressure is equivalent to only

56,085 foot-pounds,

leaving

690,825 foot-pounds

for the mechanical equivalent of the heat which disappears in overcoming the mutual attraction of the particles of the water.

On the contrary, the latent heat of expansion of a permanent gas consists almost entirely of heat which disappears in overcoming the external pressure, that which disappears in overcoming the mutual attraction of the particles of the gas being comparatively very small; in fact, in all practical calculations respecting air-engines, the latter quantity may be altogether neglected without sensible error, and the latent heat of expansion of the air treated as the exact equivalent of the mechanical work performed by it in the act of expanding.

For example,—the product of the volume, in cubic feet, of one pound of air, at the temperature of 650° Fahrenheit, by its pressure in pounds per square foot, is 59,074 foot-pounds. If that pound of air be expanded under pressure, to $1\frac{1}{2}$ times its original volume, and be still maintained at the constant temperature of 650°, by being supplied with heat from an external source, the work performed by it in expanding, will be $59{,}074 \times$ hyperbolic-logarithm of $1\frac{1}{2} = 23{,}953$ foot-pounds; and this quantity will also be sensibly equal to the mechanical equivalent of the heat supplied, and which disappears during the expansion.

In considering the performance of any thermo-dynamic engine, it is evident that the heat which disappears in producing increase of volume under pressure, is to be regarded as the real source of power; as it is a portion of this heat which is actually converted into mechanical work, while the heat expended in producing elevation of temperature, produces merely a *tendency* to the development of power.

6. *Mode of Operation of Thermo-dynamic Engines in general.*—The mode of operation of an elastic substance in performing mechanical work by the agency of heat, reduced to its simplest form, consists in the continued repetition, either

upon the same portion of the substance, or upon a succession of equal and similar portions, of a cycle of four processes, which, taken together, constitute a single stroke of the engine.*

Process A. The substance is raised to an elevated temperature. This process may or may not involve an alteration of volume.

Process B. The substance, being maintained at the elevated temperature, increases in volume and propels a piston, or something equivalent to a piston. During this process heat disappears, and a supply of heat from without is provided equal in amount to the heat which disappears; so that the temperature does not fall.

Process C. The substance is cooled down to its original low temperature. This process, like the process A, may or may not be accompanied by a change of volume.

Process D. The substance, being maintained at its depressed temperature, is compressed, by the return of the piston, to its original volume. During this process, heat is produced; and in order that it may not elevate the temperature of the substance, and give rise to an increased pressure, impeding the return of the piston, it must be abstracted as quickly as produced, by some external means of refrigeration.

The substance, being now brought back to its original volume and temperature, is ready to undergo the cycle of processes anew, and so on *ad infinitum;* or otherwise, it is rejected, and a fresh portion of the substance employed for the next stroke. When the latter is the case, the operation of expelling the substance from the engine into the atmosphere, by the return of the piston, sometimes takes the place of the process D.

Sometimes, either of the processes, B, C, or D, is the first in the order of time. The cycle of processes, however, preserves the same order of rotation.

During the cycle of processes which has been described, the working substance alternately increases and diminishes in volume, in contact with a moving piston. During the increase of volume, the pressure of the substance against the piston

---

* This cycle of processes was first described by Carnôt (Reflexions sur la puissance motrice du feu); but his conclusions were vitiated by the assumption of the substantiality of heat.

communicates to the latter mechanical power. During the diminution of volume, on the contrary, the piston expends mechanical power in compressing the working substance. The increase of volume takes place at a higher temperature, and therefore at a higher pressure, than the diminution of volume; consequently, the mechanical power communicated to the piston, exceeds that taken away from it. The surplus is the power of the engine, available for performing mechanical work.

7. *Efficiency of Thermo-dynamic Engines.*—The efficiency of a thermo-dynamic engine is the ratio which the available power bears to the mechanical equivalent of the whole heat expended.

If it were possible to construct an engine such, that the heat communicated to the working substance should entirely disappear, the power produced by that engine would be the exact equivalent of the heat expended: that is to say, 772 foot-pounds for each British unit of heat; and its efficiency would be represented by unity. According to the average estimate already stated, it would produce power to the amount of 6,000,000 foot-pounds for each pound of coal consumed; and, as a horse-power is 1,980,000 foot-pounds per hour, the consumption of coal would be 0·33 lb. per horse-power per hour. It is impossible, however, by any engine, to realize anything approaching to this degree of efficiency. This arises from two causes:—*first,* the *necessary loss* of heat, depending on the limits of temperature between which the engine works, according to a law which has been already referred to, and which will shortly be stated; and, *secondly,* the *waste* of heat and power arising from the engine's not fulfilling exactly the conditions prescribed by theory. When the *necessary loss* of heat alone is taken into account, the efficiency as determined by calculation, may be called the *Theoretical Maximum Efficiency* of the kind of engine under consideration. When the *waste* of heat and power is also allowed for, the result is the *actual efficiency* of the engine.

8. *Theoretical Conditions of Maximum Efficiency.*—The latent heat of increase of volume at an elevated temperature, being the direct source of the power of a thermo-dynamic engine, it is obvious, that, *cæteris paribus,* the more we reduce

the other part of the expenditure of heat, namely, the heat which is expended in elevating the temperature of the working substance, the more nearly shall we attain to the maximum theoretical efficiency of the engine. It is theoretically possible to produce the required elevation of temperature, without any expenditure of heat. This is to be accomplished in two ways:—either, by elevating the temperature of the substance by compression during the process A of the cycle: the power required for effecting such compression being obtained, during the process C, by depressing the temperature of the substance entirely by expansion; or otherwise, by storing up in a mass of some solid conducting material (called an economizer or regenerator), the heat given out by the working substance, while its temperature is being depressed, during the process C, and employing the heat, so stored up, to produce the required elevation of temperature during the process A. This method of economizing heat was invented in 1816, by the Rev. Robert Stirling.

By one or other of those methods, it is theoretically possible to limit the expenditure of heat in a thermo-dynamic engine to that amount which disappears during the process B of the cycle, in producing increase of volume at an elevated temperature. The heat which reappears during the process D, by the compression of the working substance at a low temperature, and which is carried away by refrigeration, constitutes the *necessary loss of heat;* and, if this be deducted from the whole heat expended, the remainder will be the theoretical maximum value of the heat which is permanently converted into mechanical power, and its ratio to the whole heat expended will be the theoretical maximum efficiency of the engine.

9. *Absolute Temperatures.*—The theoretical maximum efficiency of a thermo-dynamic engine, depends upon what are called the *absolute temperatures* of the working substance, during the second and fourth processes of the cycle; that is to say, the absolute temperatures at which heat in a theoretically perfect engine is received and abstracted respectively. Absolute temperatures are measured by the product of the pressure and volume of a given weight of a given perfect gas. A perfect gas is one in which the mutual attraction of the particles is insensible.

The *Absolute Zero of Heat* on a perfect-gas thermometer, is that point on its scale which corresponds to total absence of heat; and from this point absolute temperatures are understood to be reckoned, in the law stated in the next article.

According to the latest determination, from the experiments of Messrs Joule and Thomson, the *Absolute Zero of Heat* does not differ by any amount appreciable in practice from the *Absolute Zero of Pressure*, being the temperature at which (if it were possible for a perfect gas to preserve its properties at so intense a degree of cold) the product of its pressure and volume would be reduced to nothing; and this point is 493° of Fahrenheit below the temperature of melting ice; that is to say, $493° - 32° = 461°$ below Fahrenheit's ordinary zero.*

10. THE LAW OF THE MAXIMUM EFFICIENCY OF THERMODYNAMIC ENGINES is expressed by the following proportion:—

As the absolute temperature of receiving heat
is to the difference between the absolute temperatures
of receiving and discharging heat,
so is the whole heat received
to the portion of heat permanently converted into mechanical power;

that is to say,

so is unity to the efficiency of the engine.

This proportion may be otherwise expressed, as follows:—

As the absolute temperature of receiving heat
is to the absolute temperature of discharging heat,
so is the whole heat received
to the necessary loss of heat.†

---

* The product of the volume in cubic feet of one pound of atmospheric air, by its pressure in pounds on the square foot, at the temperature of melting ice, is 26,214 foot-pounds. The corresponding product, at any other temperature, is found with a degree of accuracy sufficient for practical purposes, by multiplying the absolute temperature by $\frac{26,214}{493} = 53{\cdot}172$ foot-pounds per degree of Fahrenheit. In the detailed investigations already referred to, the absolute zeros of heat and of pressure have had various positions assigned to them as the most probable, within a range of about 4° Fahrenheit, according to the degree of precision of the experimental data, existing at the periods when the several papers were written. This range of variation, however, is not sufficient to cause any error of practical importance in calculations respecting engines.

† There are other forms in which this law might be expressed; but those

To illustrate the above law, the following table is added, showing four examples of the efficiencies of theoretically perfect engines working between limits of temperature to which there will be occasion to refer in the sequel: the 7th column shows, in each case, the maximum theoretical duty of a pound of coal, supposing, as before, that the whole available heat of its combustion is equivalent to 6,000,000 foot-pounds: the 8th column shows, for each example, the corresponding minimum theoretical consumption of coal per horse-power per hour: the limits of temperature chosen in the five examples are respectively as follows:—

(1.) The limits of temperature for a condensing steam-engine, with a pressure of 42 lb. per square inch in the boiler, and 2·9 lb. per square inch in the condenser: (in every instance in this paper in which a pressure is mentioned, it is to be understood to mean the *total pressure* and not the *excess* above the pressure of the atmosphere).

(2.) The limits of temperature for a non-condensing steam-engine with a pressure of 153 lb. per square inch in the boiler.

(3.) A probable estimate of the limits of temperature of Ericsson's air-engine of 1852.

(4.) The same for Stirling's air-engine, and also for that of Napier and Rankine.

The actual efficiency of these engines will be considered in another part of this paper.

Examples of Maximum Theoretical Efficiency.

Nos. of Examples.	Temperatures in degrees of Fahrenheit.				Maximum theoretical Efficiencies.	Maximum theoretical Duties of one pound of Coal, ft.-lb.	Minimum theoretical Consumption of Coal per horse-power per hour, lb.
	Ordinary.		Absolute.				
	Higher.	Lower.	Higher.	Lower.			
1	270	140	731	601	$\frac{130}{731} = 0{\cdot}178$	1,067,000	1·86
2	360	212	821	673	$\frac{148}{821} = 0{\cdot}180$	1,080,000	1·83
3	480	100	941	561	$\frac{380}{941} = 0{\cdot}404$	2,424,000	0·82
4	650	150	1111	611	$\frac{500}{1111} = 0{\cdot}450$	2,700,000	0·73

stated above are the most readily applicable to the performance of engines worked by heat, and are therefore to be preferred in a paper such as the present. See Trans. Royal Soc., Edin., vol. xx. (1850 to 1853), and Phil. Trans., 1854.

Carnôt was the first to perceive, that the maximum effect of the expenditure of a given quantity of heat in a thermo-dynamic engine must be a func-

In order to show the manner in which the pressure and volume of elastic substances vary, in producing the maximum theoretical efficiency of a thermo-dynamic-engine, so as to verify in every case the general law, a supplement is added to this section, containing detailed computations for three examples of theoretically perfect engines: viz., a steam-engine working between 270° and 140°, an air-engine working between the same temperatures, and an air-engine working between 650° and 150°.

11. As the law above stated is true for all substances whatsoever in all conditions, it is obvious that, in a purely theoretical point of view, the only reason for preferring any one substance to another, as the agent in converting heat into mechanical power, is the greater ease and safety of causing it to expand by heat at a high temperature. In this point of view, permanent gases are preferable to vapours rising from liquids; for the density of a permanent gas can be regulated at pleasure so as to limit its pressure at any temperature, how elevated soever, to a safe and manageable amount; whereas a given vapour, while in contact with its liquid, has but one possible density for each given temperature, and consequently but one possible pressure; and as the pressures of the vapours of all easily obtainable fluids increase very rapidly with the temperature, it would be unsafe to use vapours at temperatures at which it is safe and easy to use permanent gases. For example, at the temperature of 650° Fahr. (measured from the ordinary zero), a temperature up to which air-engines have actually been worked with ease and safety, the pressure of steam is 2100 pounds upon the square inch; a pressure which plainly renders it impracticable to work steam-engines with safety at that temperature.

Supplement to Section I.—A. *Example of the Computation of the power produced by the combustion of one pound of Coal in a theoretically perfect Steam-Engine, working between the temperatures of* 270° *and* 140° *of Fahrenheit.*

Data.

Mechanical equivalent of the whole available heat obtained by the combustion of 1 lb. of coal, 6,000,000 ft.-lb.

tion of the temperatures of receiving and discharging heat: but the hypothesis of the substantiality of heat misled him as to the nature of the function.

	In Boiler.		In Condenser.
Temperatures (ordinary scale),	270°	Fahr.	140°
Absolute temperatures,	731°		601°
Pressures,	lb.		lb.
Per square inch,	41·93		2·89
Per square foot,	6038·		415·7

	Cubic feet.
Volume of one pound of steam in the boiler,	9·852
	ft.-lb.
Latent heat of evaporation of one pound of steam (mechanical equivalent),	715,800

Computation of the Maximum Theoretical Duty of one pound of Coal by the General Law.

Theoretical maximum efficiency, $\frac{270^\circ - 140^\circ}{731^\circ} = \frac{130^\circ}{731^\circ} = 0{\cdot}178$

Duty of one pound of Coal, $\frac{130}{731} \times 6{,}000{,}000 = 1{,}067{,}000$ as in Example I. of the table in article 10.

Computation of the Maximum Theoretical Duty of one pound of Coal, introducing the changes of pressure and volume undergone by the Steam.

Water evaporated by one lb. of coal,

$$= \frac{\text{available heat of combustion}}{\text{latent heat of one lb. of steam}} = \frac{6{,}000{,}000}{715{,}800} = 8{\cdot}382 \text{ lb.}$$

Ratio of expansion required to enable the steam to produce its maximum effect, 10·774.

The detailed computation of this ratio is too tedious to be inserted here. The method pursued is fully explained in the Philosophical Transactions for 1854, Part I.

	per lb. of water.	per lb. of coal.
	cubic feet.	cubic feet.
Space filled by steam at full pressure,	9·852	82·579
at the end of the expansion,	106·04	888·82
= space traversed by the piston.		

Effect of one pound of steam, $\overset{\text{ft.-lb.}}{715{,}800} \times \frac{130}{731} = \overset{\text{ft.-lb.}}{127{,}297}$.*

---

* This quantity consists of the total action of the entering and expanding steam, on one side of the piston, diminished by the action of the steam which

Effect of one pound of coal, $127{,}297 \times 8{\cdot}382 = \overset{\text{ft.-lb.}}{1{,}067{,}000}$ as before.

Mean effective pressure during the whole action of the steam, $= \frac{\text{effect}}{\text{space}} = \frac{127{,}297}{106{\cdot}04} = \frac{1{,}067{,}000}{888{\cdot}82} = 1200{\cdot}46$ lb. per square foot $=$ 8·34 lb. per square inch.

Coal per horse-power per hour.

$\frac{1{,}980{,}000}{1{,}067{,}000} = 1{\cdot}86$ lb., as in the 1st example of the table in article 10.

B. *Example of the Computation of the power produced by the combustion of one pound of Coal in a theoretically perfect Air-Engine, working between the temperatures of* 270° *and* 140° *of Fahrenheit.*

(The object of the following computation is not to exemplify the mode of working of any existing or proposed air-engine, but simply to illustrate the fact, that the maximum theoretical efficiency of thermo-dynamic engines is the same when the limits of temperature are the same, of what nature soever the working substance may be.

It is also to be observed, that the *maximum theoretical duty* of one pound of coal in an air-engine is independent of the rate of expansion of the air, and of its density and pressure. The rate of expansion affects the weight of air which must be employed to perform a given duty, and the densities and pressures affect the size of the receivers and cylinders required to contain that weight of air.

If definite values, therefore, are assumed for those quantities in the following calculations, it is only for the sake of fixing the ideas, and giving numbers instead of algebraical symbols.)

Data.

Mechanical equivalent of the whole available heat obtained

---

is being condensed on the other side, and also by the power consumed in producing, by the forcible compression of part of the steam into the liquid state, a quantity of heat sufficient to raise the temperature of the water from 140° to 270° Fahrenheit.

by the combustion of one pound of coal (as before) 6,000,000 foot-pounds.

Ratio of expansion of air, . . $1 : 1\frac{1}{2}$

The air is alternately expanded and compressed in this ratio.

	During Expansion.	During Compression.
Temperatures (ordinary scale),	270° Fahr.	140°
Absolute temperatures, .	731 ...	601

Product of the volume of one pound of air in cubic feet by its pressure in pounds on the square foot at 32° Fahr., 26,214 ft.-lb.

Computation of the Maximum Theoretical Duty of one Pound of Coal by the general law.

Theoretical maximum efficiency, $\frac{270° - 140°}{731} = \frac{130}{731} = 0{\cdot}178$

Duty of one pound of coal, $\frac{130}{731} \times 6{,}000{,}000$ ft.-lb. = 1,067,000 ft.-lb., as in Example I. of the table in article 10.

Computation of the Maximum Theoretical Duty of one Pound of Coal, introducing the changes of Pressure and Volume of the Air.

Product of the pressure and volume of one pound of air at the temperature of 270°, $26{,}214 \times \frac{270 + 461}{32 + 461} = 26{,}214 \times \frac{731}{493} =$ 38,869 ft.-lbs.

Power developed by one pound of air during its expansion at 270° Fahr. to one and a half times its original volume, being also the mechanical equivalent of the heat expended to produce that expansion.

$$38{,}869 \times (\text{hyp. log. } 1\tfrac{1}{2} = 0{\cdot}4054652) = 15{,}760 \text{ ft.-lb.}$$

Weight of air which is expanded to one and a half times its volume at 270° Fahr. by the combustion of one pound of coal,

$$\frac{6{,}000{,}000}{15{,}760} = 380{\cdot}705 \text{ lb.}$$

Pressures and volumes of the air at different periods, supposing the greatest pressure to be 120 lb. per square inch.

	Pressures.		Volumes.	
	lb. per sq. inch.	lb. per sq. foot.	cubic feet per lb. air.	cubic feet per lb. coal.
At the beginning of the expansion, .	120	17,280	2·2494	856·358
At the end of the expansion, .	80	11,520	3·3741	1284·537
Space through which the air expands, . = space traversed by the piston.			1·1247	428·179

	Mean Pressures.		Power = Mean Pressure × Space.	
	lb. per sq. inch.	lb. per sq. feet.	in ft.-pounds. per lb. air.	per lb. coal.
Mean pressure and power during the expansion, .	97·3	14012·88	15,760	6,000,000
Deduct mean pressure and power during the compression, $= \frac{601}{731}$ of the above, .	80·0	11520·85	12,957	4,933,000
Effective mean pressure and power, $\frac{130}{731}$ . .	17·3	2492·03	2,803	1,067,000

The calculations A and B illustrate the fact, that the maximum theoretical effect of one pound of coal between a given pair of temperatures is the same, whether the working substance be air or steam.

C. *Example of the Computation of the Power produced by the Combustion of One Pound of Coal in a theoretically perfect Air-Engine, working between the temperatures of* 650° *and* 150° *of Fahrenheit.*

Data.

Mechanical equivalent of the whole available heat obtained by the combustion of one pound of coal (as before), 6,000,000 foot-pounds.

Ratio of expansion of air, 1 : $1\frac{1}{2}$.

	During Expansion.	During Compression.
Temperatures (ordinary scale),	650° Fahr.	150°
Absolute temperatures, .	1111° ...	611°

Product of the volume of one pound of air in cubic feet by its pressure in pounds per square foot at 32° Fahr., 26,214 foot-pounds.

Computation of the Maximum Theoretical Duty of One Pound of Coal by the general law.

Maximum theoretical efficiency, $\frac{650 - 150}{1111} = \frac{500}{1111} = 0{\cdot}45$

Duty of one pound of coal, $\frac{500}{1111} \times 6{,}000{,}000 = 2{,}700{,}000$ ft.-lb., as in Example IV. of the table in Article 10.

Computation of the Maximum Theoretical Duty of One Pound of Coal, introducing the Changes of Pressure and Volume of the Air.

Product of the pressure and volume of one pound of air at the temperature of 650°.

$$26{,}214 \times \frac{650 + 461}{32 + 461} = 26{,}214 \times \frac{1111}{493} = 59{,}074 \text{ ft.-lb.}$$

Power developed by one pound of air during its expansion at 650° Fahr. to 1½ times its original volume, being also the mechanical equivalent of the heat expended to produce the expansion.

$$59{,}074 \times (\text{hyp. log. } 1\tfrac{1}{2} = 0{\cdot}4054652) = 23{,}953 \text{ ft.-lb.}$$

Weight of air which is expanded to 1½ times its volume at 650° Fahr. by the combustion of one pound of coal.

$$\frac{6{,}000{,}000}{23{,}953} = 250{\cdot}5 \text{ lb.}$$

Pressures and volumes of the air at different periods, supposing the greatest pressure to be 120 lb. per square inch.

	Pressures.		Volumes.	
	lb. per sq. inch.	lb. per sq. foot.	cubic feet. per lb. air.	cubic feet. per lb. coal.
At the beginning of the expansion, .	120	17,280	3·4186	856·358
At the end of the expansion, .	80	11,520	5·1279	1284·537
SPACE through which the air expands, . = space traversed by the piston.			1·7093	428·179

	Mean Pressures.		Power = Mean Pressure × Space.	
			in ft.-lb.	
	lb. per sq. inch.	lb. per sq. foot.	per lb. air.	per lb. coal.
Mean pressure and power during the expansion, .	97·3	14012·88	23,953	6,000,000
Deduct mean pressure and power during the compression $= \frac{611}{1111}$ of the above,	53·5	7707·09	13,173	3,300,000
Effective mean pressure and power, $\frac{500}{1111}$	43·8	6305·79	10,780	2,700,000

Theoretical minimum consumption of coal per horse-power per hour.

$$\frac{1,980,000}{2,700,000} = 0·73 \text{ lb.}$$

as in Example IV. of the table in article 10.

Synoptical Table of the preceding Examples.

Reference.	Working substance.	Temperatures.		Effective mean pressure, lb. per square ft.	Spaces	Effects
		Ordinary Fahr.	Absolute Fahr.		Per lb. of coal.	
					Cubic feet.	Foot-pounds.
A	STEAM (maximum pressure 41·93 lb. per square inch = 6038 per square foot).	° 270 & 140	° 731 & 601	1200·46	888·82	1067000
B	AIR (maximum pressure 120 lb. per inch = 17,280 per square foot).	270 & 140	731 & 601	2492·03	428·179	1067000
C	AIR (maximum pressure same as above).	650 & 150	1111 & 611	6305·79	428·179	2700000

A detailed mathematical investigation of the theory of air-engines, with and without regenerators, is contained in the third and fourth sections of a paper on Thermo-dynamics in the Philosophical Transactions for 1854, Part I., together with some numerical illustrations.

Theoretical investigations of the duty of air-engines of different forms are contained in a paper by Mr Joule (Phil. Trans., 1851), and in a series of papers in the American Journal of Science for 1853 and 1854, by Professor F. A. P. Barnard, the first American author, so far as I know, who has aided in the development of the consequences of the dynamical theory of heat.

SECTION II.—ON THE ACTUAL EFFICIENCY OF THERMO-DYNAMIC ENGINES: OF STEAM-ENGINES IN PARTICULAR.

12. *Causes of Waste of Heat and Power.*—In considering the waste of heat and power which constitutes the difference between the actual performance and the maximum theoretical performance of engines worked by heat,—as the object now in view is to compare different kinds of engines together, it is not necessary to take into account those causes of loss of power which either are or might be made nearly alike in all kinds of engines, such as the friction of the machinery; those causes alone will be considered which affect the relation between the expenditure of heat and the action of the working elastic substance upon the piston,—in other words, the *indicated* power of the engine; and from these causes will be further excepted the waste of power in forcing the working substance through narrow valve-ports and passages, as this kind of waste arises only from an error in mechanism. As thus restricted, the causes of waste of heat and power may be divided into five classes—*first,* Imperfect communication of heat from the burning fuel to the working substance; *second,* Imperfect abstraction of the heat, which constitutes the *necessary loss* explained in the preceding section; *third,* The communication of heat to or from the working substance at improper periods of the stroke; *fourth,* Any expenditure of heat in elevating the temperature of the working substance; *fifth,* Imperfect

arrangement of the series of changes of volume and pressure undergone by the working substance during the stroke. The fourth and fifth causes of waste are often connected with each other.

13. *Application to the Steam-Engine.*—In the steam-engine the first cause of waste of heat exists when the boiler presents an insufficient surface to the products of combustion, and may be considered to be almost completely removed in tubular boilers of the best construction when properly worked. It is well known that, with such boilers, the consumption of fuel per horse-power per hour is about one-fifth of what it has in some instances been ascertained to be where boilers of insufficient surface have been employed. The second cause of waste exists where the condensation is imperfect. The third cause of waste, where the cylinder, steam-passages, and boiler are exposed to the loss of heat by conduction and radiation.

As the means of indefinitely diminishing the waste of heat in steam-engines from those three causes are already to a great extent practised, it is unnecessary here to refer to them farther.

14. *Action of the Steam in a perfect Steam-Engine.*—To understand the mode of operation of the fourth and fifth causes of waste in the steam-engine, let us consider what the action of the steam in a theoretically perfect engine ought to be. We shall commence with the process B of the cycle constituting the stroke, described in article 6. An assigned portion of water being at the required temperature of evaporation, is converted into steam at that temperature, and at a pressure depending on that temperature, by the expenditure of a certain amount of heat, called the *latent heat of evaporation*, which also depends on the temperature. The steam being admitted to the cylinder, propels the piston before it; and when the assigned portion of water has been thus admitted in the form of steam, the communication with the boiler is shut. This completes the process B.

The steam now, without receiving or discharging any heat, expands: during this expansion its temperature falls by the conversion of heat into mechanical power; the pressure, of course, diminishes at the same time: this expansion ceases when the pressure and temperature of the steam have fallen to the degree fixed for its condensation. This completes the process C. During the process D the piston returns, and a portion of the steam is liquefied by contact with some cold conducting substance, which abstracts the heat generated by its liquefaction, so as to maintain it at the fixed temperature and pressure. The process D ought to stop in time to leave a portion of uncondensed steam sufficient for the process A, now about to be described. The water and steam being now prevented from receiving or discharging heat by conduction, the piston continues its return stroke, and forcibly compresses the remaining portion of steam into the liquid state. This constitutes the process A; and the portion of steam so condensed ought to be just sufficient, by the heat generated by its liquefaction, to elevate its own temperature, as well as that of the water previously liquefied, to the original temperature of evaporation, so that the entire portion of the water employed may be in every respect in the same condition as it was at the beginning of the cycle of processes B, C, D, A, which may be repeated *ad infinitum*. Such an engine would fulfil the conditions of maximum theoretical efficiency; for the elevation of the temperature of the water would be effected without expenditure of heat, and the only heat expended would be the latent heat of evaporation: those results being produced by the proper arrangement of the changes of volume and pressure undergone by the working substance during each stroke.

15. *Impracticability of such a perfect Steam-Engine.*—It is impossible to fulfil wholly in practice the conditions prescribed in the preceding article. To show the nature of the obstacles, let us begin with the process A. The forcible compression of a certain proportion of the steam into the liquid state would not only cause a very inconvenient degree of inequality in the action upon the piston at different periods of the stroke, but it is difficult to conceive any mechanism by which it could be effected in practice. The steam must therefore be wholly liquefied during the process D, and the temperature of the feed-water must be raised from the point of condensation to that of evaporation by expenditure of heat. A certain amount of heat is thus wasted; at the same time the

power which would have been expended in compressing the steam is partly saved; but the saving of power bears a small proportion to the mechanical equivalent of the heat wasted.

The amount of waste thus occasioned is comparatively unimportant in practice, provided it be not increased by unskilful methods of heating the feed-water; for, under ordinary circumstances, the heat required for that purpose seldom exceeds one-seventh part of the latent heat of evaporation, and it may be considered to reduce the efficiency of the engine below the theoretical maximum by about one-sixteenth.

Another and a more important point in which the conditions prescribed by theory cannot be exactly fulfilled, is the extent of the expansion during the process C: if this expansion were carried in practice down to the pressure of condensation, the cylinder and every part of the engine would be bulky, heavy, and costly, and the action of the steam upon the piston, during the latter portion of the stroke, would be so feeble as to cause an unsteadiness of motion unsuitable for the driving of machinery. The expansion, therefore, cannot be fully carried out. The diminution of efficiency from this cause depends upon the extent to which the expansive working is carried. Should the expansive working be wholly omitted, the efficiency may be reduced to one-third or one-fourth of its theoretical value, or even less, according to circumstances.

16. *Actual Efficiency of well-constructed Steam-Engines.* —In single-acting engines for pumping water, in which the difficulties of employing a great extent of expansive working are the least, the actual efficiency has already, in some cases, attained a value nearly approximating to its maximum theoretical value. In double-acting engines, however, so long a range of expansive working cannot be employed; and their ordinary average consumption of coal, when skilfully made and worked, is four pounds per horse-power per hour, the coal being of the evaporating power already specified. This corresponds to an efficiency represented by 0·0825, being about 0·46 of the theoretical maximum.

Considering that the causes of waste of heat and power in the steam-engine are, as has been already explained, incapable of being wholly removed in practice, it may be estimated that the greatest amount of actual efficiency to be expected in double-acting steam-engines by any probable improvement, is about three-fourths of the theoretical maximum, or 0·133,—corresponding to a consumption of coal, calculated as before, of $2\frac{1}{2}$ lb. per horse-power per hour.

SUPPLEMENT TO SECTION II.—*On the Steam-and-Ether Engine of M. du Trembley.*

(16 A.) This engine exemplifies one means of diminishing that difficulty attending the fulfilment of the conditions of theoretical perfection in the steam-engine, which arises from the impracticability of expanding the steam until its pressure has fallen to that corresponding to a low temperature of condensation.

Instead of carrying the expansion of the steam to the great extent required by theory, it is carried to such an extent only as is convenient in practice. The steam is then liquefied at the pressure attained at the end of its expansion, and the heat given out during its liquefaction is employed to evaporate ether, which works an auxiliary engine. By this process, after the expansion of the steam has been carried to a certain extent, *vapour of ether* is in fact *substituted for the steam* and made to perform the remainder of its work in its stead; and as the vapour of ether, at a given temperature, exerts a higher pressure and occupies a less volume than steam does, the cylinder of the auxiliary ether-engine occupies much less space, and gives a more steady action than would be required for the performance of the same work by continuing the expansion of the steam.

The maximum theoretical efficiency of the steam-and-ether engine is the same with that of any other thermo-dynamic engine working between the temperature of evaporation of the water, and that of liquefaction of the ether.

Its advantage consists in obtaining a nearer approximation to that theoretical efficiency within given limits as to the bulk and cost of the engine, than is practicable with an engine worked by steam alone.

SECTION III.—ON THE ACTUAL EFFICIENCY OF AIR-ENGINES.

17. As the object of this paper, in referring to the actual performance of previous air-engines, is to illustrate the waste by which that performance falls short of the theoretical maximum, I shall refer to those engines only which have actually been at work, and the details of whose performance have been made public with tolerable precision, namely, the engine of the Messrs Stirling, and that of Captain Ericsson, which latter was used for marine propulsion about the year 1852.

18. *Stirling's Air-Engine.* — In describing generally the air-engine which was invented by the Rev. Robert Stirling in the year 1816, and improved by him and Mr James Stirling at subsequent periods, it will be sufficient to speak as of a single-acting engine only; a double-acting engine having simply a similar apparatus for each side of the piston.

Suppose a cylindrical cast-iron air-receiver, of sufficient strength to be safe with a working pressure of sixteen atmospheres, with a convex hemispherical bottom, and a concave hemispherical top, to be placed in a vertical position over a flue connected with a furnace, but screened from the radiant heat; the hemispherical bottom of this receiver constitutes the surface for the reception of heat; I believe it was 3 inches thick in the engine last erected. Within this vertical receiver there is a hollow metal plunger, filled with some non-conducting substance, and capable of being moved up and down by means of a rod. This plunger is of precisely the same form with the receiver, but considerably less in height, and somewhat less in diameter. The effect of raising this plunger is to displace the air from the upper part of the receiver, and to send it down to the bottom, where it is exposed to heat; the air passing through the space between the plunger and the sides of the receiver: the effect of lowering the plunger is to cause the air to return to the top of the receiver. In the interior of the uppermost part of the receiver is a coil of small tubes, in which cold water is made to circulate, and amongst which the air must pass whenever it is displaced. Lower down, and occupying the annular space between the plunger and the receiver, are a number of parallel vertical plates of metal or glass, with narrow interstices between them, through which the air must pass on its way up or down. This system of plates is called the *Economizer* or *Regenerator;* its object being one which has already been explained in article 8, namely to store up the heat given out by the air during the process C, when its temperature is being lowered, and to give back the same heat to the air during the process A, so as to raise its temperature. Lower still, the receiver has an internal false bottom, pierced with many small holes, through which also the air must pass, and whose effect is to bring every part of the air into close contact with the heated iron bottom of the receiver. Suppose, further, that this receiver communicates at its top, through a sufficiently wide passage or nozzle, with the lower end of a working cylinder containing the piston; the receiver and cylinder are, in the first place, filled with compressed air, of any required density, by means of a small forcing-pump. As the same mass of air is used over and over again, this pump has to be subsequently worked to no further extent than is necessary to supply the loss of air by leakage, which has always been found to be extremely small. A pump is also required to keep up a stream of cold water through the coil of tubes before mentioned.

19. *Mode of operation of Stirling's Air-Engine.*—Suppose the piston to be at the bottom of the cylinder, and the plunger at the bottom of the receiver, the mass of air in the receiver is now at the top amongst and near the cold-water tubes, and its temperature is low. Let the plunger now be partially raised, part of the air is forced down through the economizer into the space between the outer and inner bottoms of the receiver, and through the holes of the inner bottom, into the space below the plunger. In passing over the heated bottom of the receiver, it has, in the first place, its temperature raised by the reception of heat from the furnace. At this point the cycle of processes formerly described may be held to begin.

Process B.—The air below the plunger receives an additional supply of heat from the furnace, which disappears in expanding it. The air below the plunger, in the act of expanding, lifts up the plunger and the mass of air above it, which latter mass of air, passing through the nozzle, lifts the piston.

Process C.—The plunger descends and forces the air below it through the holes of the inner bottom, and through the metal or glass plates of the economizer, which absorb, more or less completely, the sensible heat of the air. This air, by passing amongst the cold-water tubes, enters the space above the plunger. Should it leave the economizer at a temperature higher than that of the cold-water tubes, the latter abstract an additional portion of its sensible heat.

Process D.—The piston descends, compressing the whole mass of air; the heat so generated is abstracted by the cold-water tubes.

Process A.—The plunger partially rises, as before; a portion of air descends through the economizer, and recovers the heat remaining stored up there. Should its temperature, on leaving the economizer, not have attained its original elevation, the additional sensible heat required is supplied from the furnace through the bottom of the receiver.

The cycle of processes is now finished, and may be repeated *ad infinitum.*

Thus it appears that the air confined in the receiver and cylinder of Stirling's air-engine consists of two portions: one portion, which always remains above the plunger, and which serves merely as a perfectly elastic cushion, to transmit pressure and motion between the piston and the other portion of the air, and not as a means of developing power; and another portion of air, which, being driven by the plunger to the bottom and top of the receiver alternately, is successively heated, expanded, cooled, and compressed; and, as the expansion takes place at a high temperature, and the compression at a low one, more power is produced by the former than is consumed by the latter, and thus there remains a surplus of available power for the engine.* The existence of the cushion of air before-mentioned,

---

* In calculating the space to be traversed by the piston of an air-engine, in which part of the air acts as a cushion, allowance must be made for the space through which this *cushion-air* expands and contracts, with the variation of pressure, as well as for the space required for the changes of volume of the *working-air.* The total space traversed is thus increased in a certain propor-

leads to an important practical advantage; for it is this air alone which comes into contact with the cylinder, the piston, the packings of the piston and those of the plunger-rod, which are consequently never exposed to a high temperature.

It was, perhaps, mainly in consequence of this, that Stirling's engine, with its final improvements, required less oil and fewer repairs, worked with less friction, and was less liable to get out of order, when properly managed, than any steam-engine.

Stirling's air-engine employed to drive the machinery of the Dundee Foundry, was double-acting, having two receivers, one connected with the top and the other with the bottom of the cylinder. The plungers of those receivers were suspended by their rods from the opposite ends of a small beam. A reciprocating motion was given to that beam by means of a piece of mechanism which possessed a power of regulating the length of stroke of the plungers; and in its effect, though not in its construction, was analogous to the link motion.

The testimony of Mr James Stirling to the advantages of this engine was corroborated by that of the late Mr Smith of Deanston and by that of Mr James Leslie.

20. *Efficiency of Stirling's Air-Engine.*—According to Mr Stirling, the air in his engine received heat at the temperature of 650° Fahr., and discharged the lost heat at that of 150° Fahr. The fourth example of the table in Article 10 shows that the efficiency of a theoretically perfect engine, with those limits of temperature, would be 0·45, and its consumption of coal 0·73 of a lb. per horse-power per hour.

It appears that the actual consumption of coal per horse-power per hour was about 2·2 lb., being three times the consumption of a theoretically perfect engine, and corresponding to an actual efficiency of 0·15, or one-third of the maximum theoretical efficiency.

Stirling's air-engine, therefore, was more economical than any existing double-acting steam-engine,—probably indeed more economical than any possible double-acting steam-engine.

---

tion, and the mean effective pressure diminished in the same proportion; so that the mechanical effect remains unaltered.

As compared, however, with a theoretically perfect engine, working between the same temperatures, it appears that two-thirds of its expenditure of heat was wasted.

21. *Causes of waste in Stirling's Air-Engine.*—We shall now investigate the causes of waste in Stirling's air-engine according to the classification explained in article 12.

(1.) *Imperfect communication of heat from the burning fuel to the working substance.*—As the heating surface in Stirling's air-engine consisted simply of the hemispherical bottoms of the receivers, it was of the worst form possible for exposing a large surface within a given space. A steam-boiler of that form would occasion an enormous waste of fuel; it is probable, therefore, that this first cause of waste operated powerfully in Stirling's engine.

(2.) *Imperfect abstraction of the lost heat.*—It is probable that Stirling's engine was comparatively free from this cause of waste, for the cold-water tubes exposed a large surface, and were abundantly supplied with water.

(3.) *The communication of heat to or from the working substance at improper periods of the stroke.*—This cause must have operated powerfully to occasion waste of heat in Stirling's engine, for the following reason:—It is obvious, from the construction of the engine, that the air, whether being expanded or compressed, must have been continually circulating over the heated bottom of the receiver, and receiving heat through it from the furnace, at all periods of the stroke. Now it is only while the air is being expanded that the heat received by it is effective in producing power; while the air is being compressed, the heat received by it is detrimental. The heat received, therefore, by the air in Stirling's engine during at least one-half of each stroke—that is to say, probably one-half of the heat received—must have been absolutely wasted: it would be simply carried to the cold water tubes, and there abstracted, without producing any work. It is probable that, in an air-engine free from such cause of waste of heat, a much smaller extent of cooling surface would be found sufficient to abstract the lost heat.

(4.) *Expenditure of heat in elevating the temperature of the working substance.*—In the air-engine, the sensible heat of temperature is not, as it is in the steam-engine, of secondary importance. If the temperature of the air in an air-engine were elevated altogether by means of heat supplied from the furnace, the waste from this cause would be from three to four times greater than the latent heat of expansion which performs the work, and the economy of the engine would be entirely destroyed. Some persons, founding their calculations upon this supposition, have pronounced the air-engine to be necessarily wasteful and inefficient.

The sensible heat in question might be entirely produced by an additional compression of the air performed during the process A, the power employed to effect such compression being developed by an additional expansion performed during the process C, in which the temperature of the air falls. To afford room, however, for the additional expansion, the bulk of the engine would have to be increased about five-fold, which would render it inconvenient in practice, especially for propelling ships.

The process actually pursued in Stirling's engine, of storing up the sensible heat by means of the economizer or regenerator, and using it over and over again, has already been generally described. In the original engine of the Rev. Robert Stirling, the regenerator consisted simply of the sides of the receiver and plunger, the latter being covered with a network of wires, in order to increase the surface; in the engine, as improved by Mr James Stirling, it is composed of thin parallel plates of metal or glass. In Captain Ericsson's engine it consists of several sheets of wire gauze.

The efficacy of a regenerator to prevent expenditure of heat in raising the temperature of the air increases with its mass and surface; but no amount of mass and surface, how large soever, is sufficient to make it act with theoretical perfection. There is reason to believe, however, that both in Stirling's and in Ericsson's engines the masses and surfaces of the regenerators were sufficient to reduce the waste of heat, in raising the temperature of the air, to a very small quantity.

Some persons, overlooking the latent heat of expansion—the real source of power—appear at one time to have imagined that a theoretically perfect regenerator would prevent all expenditure of heat whatsoever, except losses by conduction and radiation. This amounted to representing Stirling's air-engine as a machine for creating power out of nothing, popularly called a "*perpetual motion.*" It is very probable that the promulgation of that erroneous theory may have led scientific and practical men to regard the real performances of this engine as delusive, and may have been the cause which, notwithstanding its economy as compared with steam-engines, prevented the extension of its use beyond the Dundee Foundry.

(5.) *Imperfect arrangement of the series of changes of volume and pressure.*—It is not likely that in Stirling's engine any material amount of waste arose from this cause, for the series of changes in question would be regulated by the relative motions of the piston and plungers; and those motions being susceptible of adjustment, as in the case of the piston and slide-valve of a steam-engine, would be fixed, by trial, so as to act in the manner found to be most advantageous.

From all that has been stated, it appears,—that the principal causes of waste of heat in Stirling's engine were—first, deficiency of heating surface, and, secondly, communication of heat to the air during that part of the stroke when it was not being expanded;—that the latter cause was sufficient of itself to double, or nearly to double, the theoretical consumption of fuel; that the actual consumption of fuel was triple the theoretical consumption; but that, notwithstanding such defects, the engine was economical as compared with steam-engines.

22. *Ericsson's Engine of* 1852.—In this engine the compression and expansion of the air were performed in two different cylinders, and at each stroke the air which had been used was expelled into the atmosphere, a fresh supply of air being at the same time taken in to perform the next stroke. This process of expelling the used air, and taking in fresh air corresponded to the process C of the cycle; for the air expelled being, while in the cylinder, at a high temperature, was driven through a regenerator of wire gauze, and there left its sensible heat. This mode of working involved a great practical disadvantage, especially for marine purposes; for the cylinders had to be made large enough to contain the requi-

site supply of air at the ordinary atmospheric pressure, and the engine was consequently of enormous bulk and weight as compared with its power.

To proceed to the process D: It consisted in compressing the air with which the compressing cylinder had been filled to about two-thirds of its original volume, and forcing it into a receiver or magazine for compressed air. There was no provision in the compressing cylinder for abstracting the heat produced by the compression, and a certain waste of power must have arisen from this cause, which will be again referred to in its order.

The process A consisted in opening the induction-valve of the expanding cylinder, and filling that cylinder about two-thirds full of the compressed air. In the act of entering the expanding cylinder, the air passed through the regenerator which was fixed in the nozzle, and, receiving the heat stored up there, had its temperature elevated. On the admission of the proper quantity of air, the induction-valve was closed.

The process B consisted in the expansion of the air in the expanding cylinder, the latent heat being supplied from a furnace placed directly beneath the bottom of that cylinder.

The process C was then recommenced by opening the eduction-valve, to allow the hot air to escape through the regenerator, and so on, as before.

23. *Efficiency of Ericsson's Engine of* 1852.—In calculating the efficiency of this engine, I have been guided chiefly by data contained in the report of Professor Norton (regarding him as a neutral inquirer). As nearly as I can judge, the efficiency of a theoretically perfect engine, working between the same temperatures, would be 0·404, corresponding to a consumption of 0·82 lb. of coal per horse-power per hour. According to Professor Norton, the actual consumption was 1·87 lb. of *anthracite*, being equivalent to 2·8 of bituminous coal, if 3 lb. of bituminous coal of the quality specified in this paper be taken as equivalent to 2 lb. of anthracite. This is about 3·4 times the consumption of a theoretically perfect engine, and corresponds to an actual efficiency of 0·118, being less than the maximum theoretical efficiency in the ratio of 0·295 to 1. The waste of heat and power, therefore, in Ericsson's

engine must have been very great, though it was economical of fuel as compared with steam-engines.

24. *Causes of waste of heat in Ericsson's Engine of* 1852. —(1.) *Imperfect communication of heat from the furnace to the air.*—This cause of waste of heat must have operated to a great extent in the engine in question; for the heating surface was simply the bottom of the expanding cylinder; at the same time an extensive heating surface was rendered doubly necessary by the low pressure of the air; for, as was long since shown by Dulong and Petit, the power of gases to receive and communicate heat increases with their pressure.

(2.) *Imperfect abstraction of the lost heat.*—It has already been stated that there was no provision for abstracting the heat produced in the compressing cylinder; the direct effect of this would be to cause an additional and unnecessary expenditure of power in compressing the air.

(3.) *Communication of heat to the air at improper periods of the stroke.*—This cause of waste must have operated to a considerable extent; for the air, after having performed its work, and while in the act of being discharged into the atmosphere, continued to circulate over the heated bottom of the cylinder, and must have carried away a considerable amount of heat. This heat would not be stored in the regenerator, which must have received no more heat from the escaping air than had been previously abstracted from it by the air when entering, or otherwise the temperature of the regenerator would have gone on continually rising.

(4.) *Expenditure of heat in raising the temperature of the air.*—There is reason to believe that in Ericsson's engine, as in Stirling's, the regenerator was adequate to prevent any considerable waste from this cause.

(5.) *Improper arrangement of the changes of volume and pressure.*—There is no reason to believe that any material waste arose from this cause.

It may be observed that Ericsson's engine, as well as Stirling's, was absurdly represented by some parties as a "*perpetual motion.*"

(*To be continued.*)

On the Intrusion of the Germanic Races into Europe.[*] By DANIEL WILSON, LL.D., Professor of History and English Literature, University College, Toronto. Communicated by the Author.

Dr Arnold, in that beautiful but imperfect narrative of Roman History which his lamented death arrested in its progress towards completion, after devoting a chapter to the description of the general condition of Europe at the commencement of the fourth century before the Christian era, thus concludes:—"Such was the state of the civilized world, when the Kelts, or Gauls, broke through the thin screen which had hitherto concealed them from sight, and began, for the first time, to take their part in the great drama of the nations. For nearly two hundred years they continued to fill Europe and Asia with the terror of their name; but it was a passing tempest; and, if useful at all, it was useful only to destroy. The Gauls could communicate no essential points of human character in which other races might be deficient; they could neither improve the intellectual state of mankind, nor its social and political relations. When, therefore, they had done their appointed work of havoc, they were doomed to be themselves extirpated, or to be lost amidst nations of greater creative and constructive power; nor is there any race which has left fewer traces of itself in the character and institutions of modern civilization."

We must not, however, too hastily assume the extirpation of any race, or the altogether transitory and evanescent influence of its physical or intellectual peculiarities, merely because it ceases to play an independent part as a distinct nation. To those who recognise in all its fulness the influence of primary ethnological differences on national character and institutions, it cannot be doubted that the intermixture of races has largely affected the character of nations. The ancient Pelasgic and Etruscan races have disappeared, yet probably not by extirpation, but by absorption; and perhaps contributing, in no slight degree, by their diverse ratios of

---

[*] Read before the Canadian Institute, April 1, 1854.

intermixture with Hellenic and Kelto-Italian blood, to produce the permanent differences between the two great nations of classic antiquity.

That the Keltic ethnological element has exercised no beneficial influence either on the intellectual or physical condition of medieval and modern Europe, is no less problematic. The blood of the Gaul still gives no partial hue to the complexion of Gallic France, nor can we assume that no portion of our peculiar Anglo-Saxon national character—so different, in some respects, from that of our continental Saxon congeners—is derived from the early intermixture of the Saxon and Scandinavian with the native Celtic blood. The invasion of the Anglo-Saxons, as of the Danes and Northmen, was one of warriors, not of colonists with their wives and families, and their first settlement must have involved some extent of alliance and mingling of races, such as we see taking place in our own day with aborigines whose physical and moral characteristics present a far more antagonistic diversity of aspect. But viewing the ancient Gauls as they first appear on the stage of history, unaffected as yet by those Germanic or Anglo-Saxon elements which temper

> "The blind hysterics of the Celt,"

the justice of one portion, at least, of Dr Arnold's remarks may be perceived, if we look to the transitory nature of the Keltic philological influence on our own English tongue, and consider that while, for upwards of seven centuries after the date here referred to, no other intrusion of foreign races had taken place in the British islands than the very partial military occupation by the Roman legions, yet the English language retains no grammatical or constructive elements of the ancient native Keltic or British tongues, and has so few etymological elements incorporated into its composite vocabulary, excepting such as are indirectly derived through the Latin, that the whole of such might be expunged without sensibly marring the richness and copiousness of the language. Historically speaking, the English language of the British islands stands in precisely the same relation to its ancient geographical area as the English of Canada and the United States does to this portion of its widely-diffused modern area; in neither is it the original language of any part of the countries to which it now pertains, but in both cases it has spread itself within well ascertained, though diverse periods, at the expense of earlier and more aboriginal languages, which it has displaced and superseded.

Looking, however, upon the older ethnological stock of British and European population, to which the Keltic elements of European languages and customs are traceable, it is important to consider whether the well-ascertained date of its first appearance on the stage of history above referred to, in any degree coincides with that of its earliest intrusion into Europe, or with the appearance of that other hardy barbarian stock, which, issuing at a later period from its fastnesses in the old unexplored north, swept before it, in its young strength, the decrepit vestiges of Rome's Imperial decline? In other words, I would inquire if the Keltic and Germanic races are coeval in their origin, or in their occupation of the European areas which they are found in possession of at the dawn of history?

"We can trace," says Dr Arnold, "with great distinctness the period at which the Kelts became familiarly known to the Greeks. Herodotus only knew of them from the Phœnician navigators; Thucydides does not name them at all; Xenophon only notices them as forming part of the auxiliary force sent by Dionysius to the aid of Lacedemon; Isocrates makes no mention of them: but immediately afterwards, their incursions into Central and Southern Italy on the one hand, and into the countries beyond the Danube and Macedonia on the other, had made them objects of general interest and curiosity, and Aristotle notices several points in their habits and character in different parts of his philosophical works." Like the first glimpses of the Kassiterides, or Tin Countries of Southern Britain, we discern, only vaguely and by chance incidental notices, the western Kelts, described by Herodotus as a people who "dwell without the Pillars of Hercules, and bordering on the Kynesians, who live the farthest to the west of all the nations of Europe." * Few passages of ancient his-

---

* This description Dr Latham would refer to the Kelts as Iberians, and not to the *Kelts* in the general sense in which the designation is accepted, and as it

tory convey to us a more vivid impression of the complete isolation of the diverse tribes then scattered over the European continent. The Pyrenees and the great Alpine chain, spreading eastward to the head waters of the Danube, formed, in the age of the Father of history, a barrier of exclusion for all the Transalpine races, scarcely less effectual than that which, for upwards of eighteen centuries thereafter, concealed this great antiquity, America, from the eyes of Europe. Kelts, Kymric or Gaelic, had doubtless crossed the Alps long prior to the first notice of them by Herodotus, and had established themselves in the fertile valley of the Po, as well as extended their influence far southward into the Italian peninsula. Whether, at that period, they had ever been present on any portion of the Hellenic area of Greece, may well be questioned, notwithstanding the undoubted Keltic elements recognised in the Greek language. They had, however, already passed to the south of the Pyrenees, and intermingling with the older Iberians of Spain, constituted the ancient Keltiberian population of Arragon and Valencia: unless, indeed, we are prepared to recognise in the Keltæ and Galatæ of Aristotle and Diodorus something more than varied forms of the same name; though even then, the distinction will not necessarily imply a greater one than the philologist recognises between the Keltic elements of the ancient Greek and Latin, or the ethnologist perceives to separate the modern Gael and Kymri of Great Britain.

To the Greeks of the age of Herodotus the Kelts were only known, by the chance report of some Phœnician seamen, as one among the rude tribes of the barbarian West, where the coasts of Europe intruded furthest into the mysterious Atlantic main, which was to them the aqueous boundary of the world. The Greeks of that age little suspected that these same western Kelts reached from the shores of the Atlantic

---

was understood by the Romans in the time of Cæsar. But it is not at all improbable that the population of Gallicia and the Biscayan provinces of Spain might have been purely Gallic B.C. 400, and yet that the displaced Ibéri of the south might have even crossed the Garonne in Cæsar's time. Immense displacement had taken place during the interval in the Spanish peninsula. But the name *Garonne,* like the Scottish *Garry,* is essentially Celtic and descriptive: *the rough river.*

Ocean as far as the Alps, and overflowing and sweeping round them, already occupied the valley of the Po, and extended nearly to the head of the Adriatic. "The narrow band of coast occupied by the Ligurian and Venetian tribes," says Dr Arnold, when referring to the approaching Gaulish invasion of Rome, "was as yet sufficient to conceal the movements of the Kelts from the notice of the civilized world. Thus, immediately before that famous eruption which destroyed Herculaneum and Pompeii, the level ridge which was then Vesuvius excited no suspicion; and none could imagine that there were lurking close below that peaceful surface the materials of a fiery deluge, which were so soon to burst forth, and to continue for centuries to work havoc and desolation."

But though that celebrated eruption which took place in the first century of the Christian era is the earliest on record, it is well known to the geologist that the pent-up fires of Vesuvius and Solfatara had long before overflowed the Phlegræan fields; and, in like manner, the philologist recognises, on no less indisputable evidence, the traces of earlier Keltic intrusions than that which, in the fourth century of Rome, swept like a wasting torrent over Central Italy. The attention of the members of the Canadian Institute has recently been directed to the well known Keltic element now universally recognised as forming so important a constituent part of the Latin tongue. This Professor Newman assumes to be an essentially intrusive element; but in doing so he recognises it as derived from Italian races, which, if not aboriginal, are known to us as the primitive inhabitants of well-defined areas of the Italian peninsula at the very dawn of history. Among these Keltic Italians, the Umbrians and the Sabines are specially remarkable, and the essential Celtic* character of the Sabine clanship, out of which the later Roman clients, and the whole system of Roman patron and client, patres and plebs, were

---

* For the purpose of discriminating between the undoubted modern Keltism of the Gael, Kymri, &c., of the British Isles and Bretagne, and the assumed but disputable Keltism, in this sense, of some ancient ethnological elements—*e. g.*, the Celtiberians of Spain—the term *Keltic* is employed here in reference to all ancient and purely continental elements, that of *Celtic* to all modern and British elements.

naturally developed, points to a social condition prevailing among the ancient tribes of Central Italy, and especially among the Sabines, more easily explicable by the analogies of modern Celtic clanship as it existed in Scotland down to the middle of the eighteenth century, than by any other source which history discloses to us.

Assuming, with Pritchard, Newman, and other able philological critics, the Kelticity of the Umbrians, and the Kelto-Italian character of both the Umbrians and Sabines, we are left in no doubt as to the antiquity of the Keltic ethnological element in Southern Europe. Among the primitive native Italian populations, the Umbrians were, at the earliest times, the cultivators of the soil and the builders of cities; and their ancient capital, Ameria, was one of the oldest cities of Italy. Pliny assigns the date of its foundation 381 years before that of Rome. Specimens of the language of this people have been preserved to us in the celebrated Eugubine Inscriptions, discovered at Gobbio, the ancient Iguvium, and the relation of this language to the Latin has been satisfactorily assigned by Grotefend and others. But without attempting to determine how far the famous Sabines and Latins, or the less important tribes of Piceni, Vestini, Frentani, and Marsi, which clustered around their ancient areas on the east, approximated to the Umbrian type, it is sufficient for our present purpose to know that " the primitive Latin must have Keltized itself by imbibing Umbrian," (Newman's " *Regal Rome*,") and that the Keltic element of the Latin is derived, being isolated and fragmentary, and only traceable to its etymological family groups by a reference to the surviving Celtic dialects. We are hence left in no doubt that that appearance of the Kelts or Gauls in Central Italy, B.C. 389, which Dr Arnold has characterized as their " beginning for the first time to take their part in the great drama of the nations," was by no mean their earliest intrusion into Southern Europe. Dr Latham, who is little disposed to extend the Keltic area further than the strictest evidence will sanction, and even denies the Kelticity of the element mingling with the Iberian stock to constitute the Celtibéri of Spain (*Ethnology of Europe*, p. 37), in restricting the original area of this ancient race, remarks, " I am inclined to limit the Keltic area, at its maximum extension, to Venice westwards, and to the neighbourhood of Rome southwards. But this is not enough," he adds, "they may have been aboriginal in parts which they may seem to have invaded as immigrants."—(*Man and his Migrations*, p. 169.)

It may thus be assumed, as obvious and undoubted, that the invasion of Rome and Central Italy by the Gauls was no intrusion of a new race, like the first appearance in Europe of the Huns in the fourth century, or of the Moors in the eighth century of our era. May it not, however, indicate to us other intrusions of which it was a secondary cause? My belief is that it does. It is abundantly obvious that some great cause of dismemberment and revolution was then affecting the great Keltic race. Whatever their older area may have been, we find the Kelts soon after intruding into Thrace and Illyricum, and appearing on the borders of Macedonia in the reigns of the great Philip and Alexander. They even overflow into Asia; and, for nearly two centuries, glance, meteor-like, on the pages of ancient history, the dismembered relics of an old barbarian nationality, terrible though transient in the destructive influences of its scattered fragments. This was the waning struggle of the great Keltic stock. Upwards of two thousand years have elapsed, and still the fragments of that once predominant European branch of the human family linger on the western confines of Europe, preserving to us their ancient tongue, so invaluable for all the investigations of the ethnologist; but assuredly their days are numbered, the hold of twenty centuries is at length giving way, and it seems probable that, ere many more generations have passed, the living languages of the Kymri and the Gael will exist only, like the Cornish, in grammars and vocabularies of the philologist, and in the surviving fragments of their ancient literature.

The stock by which the ancient Keltæ of Europe have been displaced, and the classic nations superseded, is the Germanic or so-called Teutonic group, of which our own Anglo-Saxon race is the most powerful and widely diffused of all its members. The intrusion of the Germanic stock into Europe lies beyond the assigned dates of ancient history; but many indications serve to show, that while the Keltic races only obtrude upon the historic arena in their decline, like some long-voyaging ship seen for the first time as it dashes amid the breakers of a strange and rock-bound coast, the Germanic races dawn upon us in their young barbarian strength, with all their national being still awaiting its development, and with the geographical arena of their historical existence occupied by the precursors whom they came to displace. Assuming, as a general rule, the uniform north-western progression of European population from the Asiatic cradle-land of the human race, to which science, no less than revelation, points, we are thence led to assign a certain relative age to races from their geographical position. In the extreme north are still found the Ugrian Fins and Laps, pertaining to a stock whose congeners abound in Asia and find their modern European representatives in the intrusive Majiars of Hungary, but who, as an ancient European stock, appear as the probable representatives of those Allophyliæ, whose existence in the north of Europe, and in Britain, in periods prior to all written history, is now generally accepted as an established truth. In like manner, the mountainous Basque region of the Pyrenees shelters the last remnant of the ancient Iberian stock, an unclassed, if not aboriginal Allophylian race; while, among the mountains of Albania—like waifs caught in the eddy of the great western stream of population—are still found the Skipetar, another unclassed race, who, for aught that can be said to the contrary, may as truly represent to us the aboriginal Pelasgi of Greece, as the Basques undoubtedly do the Ibéri of Spain. Leaving those, and coming down in point of time to the Indo-European historic races, we find the Gaelic Kelts in the extreme north-west, as in North Britain and Ireland, and in Gaul, with the Kymric and other Kelts, as the Welsh of England, and the Cimbri and even the Teutones* of the

---

* The science of Ethnology is still so much in its infancy, that it will least surprise the most zealous of its students to find its longest accepted terms called in question. Dr Latham has advanced reasons in his "Ethnology of Europe," for believing that, "instead of the ancient Kelts of Iberia having been Kelts in the modern sense of the word, the Kelts of Gallia were Iberians," *i. e.*, were a different race from the Gauls north of the Garrone. Next to the term *Celtic*, no word is better established among English, though not among continental ethnologists, than *Teutonic*, as equivalent to Germanic, and thereby

northern shores of the European mainland, all occupying the geographical positions to which the foremost intruders into the European area must have been driven by the accession of successive migrations from the east. In Greece and Italy were the Hellenic and Kelto-Italian successors of the Pelasgi, with, in the Italian peninsula, the intrusive Semitic race of the Rasena or Etruscans. In Spain were the Ibéri and Celtibéri, with also a small intrusive race: Phœnician or Punic; and those with the Phocian and Punic colonies of Masallia

contradistinguished from Keltic. The term, however, is at best arbitrary, at worst altogether false; for it is by no means improbable that the Teutones were Keltic, as it is certain that the evidence of Appian tends to show that both they and the Kỳmbri were of Gallic origin. (Vide Latham's "*Germania of Tacitus*," pp. cx., clx., clxiv.) The names *Teutones* and *Teutoni* have been mistakenly assumed as derived from the German *deutsch, teut-sch=teut-oni.* But the word signifying people, from which *deutsch* is derived, is either written, *thiud*, Anglo-Saxon *theod*, or *diut;* never *thiut*, or *theut*, still less *teut*. *Teut*, on the contrary, appears to be a Gallic syllable. We find, among the Gauls, Teutomatus (Cæs. b. 7), Teutates (Lucan), Teutomalus (Liv. Epist.). One of the Teuton chiefs was called Teutobochus or Teutobodus (Florus and Eutropius), while Pliny (v. 32) speaks of a Galatic people: Teutobodiaci. Another of the captive Teuton chiefs is named by Plutarch, Boiorix; while Livy (34, 46,) names a Boiorix of a "Regulus" among the Galli Insubres in Upper Italy. There was a weapon peculiar to the Teutons, called *cateja* (vide Virgil, b. 7, Teutonico ritu soliti vibrare cateias), which Isidor calls *Genus Gallici telie:* the termination *eja* being strictly Gallic. Among the Belgs were the Aduatici, whose name is purely Keltic, and even recals that of the Atacotti in Britain; but these Aduatici were, according to Cæsar, descendants of the Cimbri and Teutoni. Old Festus (de signif. verborum) says that the Ambrones who followed the Teutoni, were *gens Gallica.* The Kymbri themselves were anciently known as *Galli.* The oldest author mentioning them is Sallust (Bell. Jugurth., c. 114, adversorum Gallos ab ducibus nostris Q. Cæpioni et M. Manlio male pugnatum est); also the Kimbric slave sent to kill Marius at Mintuone is called *natione Gallus* by Livy (Epist. 77). The latter notices tend to show that the assertion of Strabo, or rather Posidonius (Strabo 7), afterwards repeated by Plutarch (Marius, c. 11), that the Cimbri and Cimmerii are the same, is not one to be hastily rejected, though so able and cautious an authority as Dr Latham has expressed himself as "utterly disbelieving the Cimmerii of the Cimmerian Bosphorus to have been Keltic." (*Man and his Migrations*, p. 169.) The above argument is chiefly designed, however, to justify the substitution of the term *Germanic* for that of *Teutonic*, employed by me elsewhere, and generally used in England to designate the Scandinavo-German race. Even if the Teutons can be shown to be Germanic, they were always a comparatively small and unimportant tribe, nor is the suitableness of the denomination *Germanic* disputed by any one; the supposed risk of confusion with it, in its modern political sense, has alone interfered with its adoption.

and the larger Mediterranean islands, constitute the population of Southern Europe, when the curtain first rises and reveals to us the great arena of the world's later civilization. To the north of this, our imperfect knowledge suffices to disclose the central area of the continent, lying between the Alps and the German Ocean, occupied, from the Atlantic to the head of the Adriatic, by the different branches of the Keltic stock, and thence eastward to the Euxine Sea, and along the valley of the Danube, by the Scytho-Sarmatian stock, including the whole Lithuanian and the first of the Slavonian populations, by whom so large a portion of their ancient area is still retained. Of these latter the Lettes are the most ancient: the Lithuanic being the likest of all the Indo-European tongues to the Sanskrit, the ancient sacred language of India.

As a broad ethnological sketch of the superficies of Europe at the dawn of authentic history, this is no baseless theory, but an outline of facts as well established as the nature of the imperfect evidence admits. But it will be seen that only a very slight extension of the old Ugrian area, such as is presupposed by the assumption of the Fins and Laps of Northern Europe constituting the remnant of a more widely diffused Allophylian stock, is requisite to occupy the whole of Europe, without the presence of a single branch of the Germanic stock in any of their later geographical areas. While, however, those various older races were gradually moving westward, ever pressed from behind by fresh swarms from the Asiatic hive, till the Gael overflowed from Gaul into Britain, northward into the Kimbric Chersonesus, and southward into Italy, the younger Germanic stock entering Europe by the only unguarded portal, between the southern spur of the Ural Mountains and the Caspian Sea, circa 500 v. 400 B.C. (?), found their way along the banks of the tributaries of the Vistula to the Baltic.

Besides the approach to Southern Europe by the Mediterranean, by means of which the isolated Semitic populations of Etruria, Gadir, and Tartessus, and the Phocian and other colonial offshoots of south-eastern civilization, reached its north-western shores, there are only two passages, or at most three, open to the migratory wanderers from Asia to Europe. The most southern of these, which required the navigation of the Hellespont or the Thracian Bosphorus, may be supposed to have been the course pursued by the ancient Pelasgi, or some still older southern Allophyliæ, in times lying beyond all history. This road, however, we know was early closed by the occupation of the whole of Asia Minor by Phrygians, Lydians, Lycians, Phœnicians, and other civilized and warlike people, whose presence entirely precluded the approach of any migratory horde to the shores of the Propontis. Beyond this, therefore, later migratory tribes, including, perhaps, the earliest pioneers of Keltic colonization, would find open for them the narrow passage formed by the lower valleys between the Caucasus and the Caspian Sea, and then reaching the northern shores of the Kimmerian Bosphorus, they would enter by the passage between the Carpathian Mountains and the Euxine into the fertile valley of the Danube. This road, also, in itself narrow and straightened, was closed against such nomade intruders long prior to the dawn of history, by the occupation of the whole country around the lower Danube by Scythic tribes belonging to the Thracian division. These warlike tribes were in undisputed possession of this important European area when we obtain our first glimpse of them in the pages of Homer, and no doubt can be entertained of their ability to withstand the encroachments of all later intruders.

Thus, then, at the assumed period of the immigration of the Germanic nomades, after the entire occupation of Southern and Central Europe by older races, there remained only one road open for tribes immigrating westward from Asia into Europe, through the Ural passage to the north of the Caspian Sea; and thence—the southern road through the valley of the Danube being now closed—they must have crossed the vast prairies of Russia, along the northern edge of the impenetrable forests of Volhynia and Poland, and the watershed of the Dnieper and the Vistula—the route pursued by the Huns, under Attila, in the fifth century—and thence along the tributaries of the Vistula to the Baltic. Here the ethnologist may be said to strike the trail of the first Germanic nomades. The later Cimbri or Kymri, and the younger Scytho-Sarmatians in their wake, having been obliged to pursue a north-western course till they reached the shores of the older Baltic, the Kymri, and no doubt also the Belgæ, penetrated still further to the westward, while their Scytho-Sarmatian followers remained at the Vistula. The Germanic nomades, beginning their intrusive migration long after their precursors had consolidated their power, and occupied their borders with the increased numbers of a settled population, were compelled to pursue the still more northern, but less encumbered course; while being, in the common movement towards the west, driven to the shores of the Baltic near Livonia and Esthonia, they crossed to the Islands, to Gottland, Oland, and to Scania, and there settling themselves in the great northern Scandinavian peninsula, where archæological research proves them to have displaced an older Allophylian population, they nursed their young strength, preparatory to their intrusion on the historic area of ancient Europe.

Archæological investigations contribute many valuable accessories to such ethnological inquiries, and specially tend to confirm the conclusions here advanced relative to the late arrival of the Germanic nomades in Western Europe. This is strikingly shown by the abrupt transition from the aboriginal stone relics to the evidences of the Metallurgic arts of the last Pagan period disclosed in the sepulchral depositories of Northern Scandinavia.*

Having established the Germanic nomades as a settled people in the northern peninsula still occupied by one great branch of the Germanic stock, the course pursued by them when they in turn became the aggressors is abundantly manifest, even now, on the map of Europe. Passing over into Denmark, and to a great extent displacing and dispossessing the Kymri, they entered Central Europe from that *point d'appui*, penetrating like a wedge between the Gauls and the Sarmatians, and gradually occupying the whole modern Germanic area between the Elbe and the Rhine. This is the movement which I conceive manifested itself by that overflowing of the Gauls into Central Italy, by means of which they, and thus also, indirectly, the Germanic aggressors on

---

* Vide *Prehistoric Annals of Scotland,* p. 358.

their rear, began, for the first time, to take their part in the great drama of the nations. Then it was that the Gallic population, pressed on from the north-east and confined on the west by the Atlantic, passed over into Britain; not, indeed, occupying it for the first time with a Keltic population, but intruding upon the older Keltic occupants, the Gallic Cantii, Belgæ, and others of those newer southern tribes, whose sympathy with their continental brethren first exposed their country to the aggressive arms of Rome. Few questions in ancient ethnology have been more keenly disputed than the Germanic or Keltic character of the Belgæ of Picardy; but nearly all ethnologists now agree in assuming that the Belgæ of Britain came from Belgic Gaul, and in the opinion that the continental Belgæ were Kelts. These points being assumed, all that we learn of the Belgæ from Cæsar—their warlike hardihood in maintaining the passes of the Rhine, the diversity of their dialect from the older Gauls, and the union and consanguinity recognised among themselves (Cæs. *Bell. Gall.*, XI., 4)—confirm the idea of their recent migration from the eastern shores of the Rhine, and the consequent recentness of the Germanic intrusion of which this was a product.

The same great Germanic migration from the north into the centre of Europe, pressing southward, drove a part of the intercepted Keltæ to seek an outlet down the valley of the Danube, encountering in that fertile region Illyrian and Thracian occupants, and mingling with or displacing them in that rich country, the fertility and many natural advantages of which have so often contributed to make it the theatre of contending claimants. This may account for the two names, Danube and Iser: the former the Keltic name, afterwards adopted by the Romans, while the latter was accepted by the Greeks. When Alexander the Great, in 335 B.C., moved against the Thracians, he found the Kelts already settled to the east of the Adriatic, and received offers of alliance from them, not as a recent band of strange intruders, but as the proud and ambitious aggressors, who, at a later period, under Brennus, invaded Macedonia and Ætolia, and even attacked the holy Delphic shrine. The Keltic tribes, thus cut off from the great stock, and compelled to retrace their course, not only penetrated eastward, as we have seen, into Thrace, but passed over into Asia Minor, where they peopled Galatia; while, if we hold to the true Kelticity of the Keltic element of the Celtibéri of Spain, we may account for a similar overflow of the Gallic Kelts into the Iberian peninsula.

Thus we have the non-Indo-Germanic Phœnician, Punic, Etruscan, and other Semitic elements, passing by the southernmost route, from the shores of the Levant, into Southern Europe, and consequently not diffused as from a common centre, but occupying isolated and widely scattered positions. The oldest branch of the great Indo-European family of nations, the Gallic Kelts, follows by the southern land passage, preceding the classic races, and contributing to them a large portion of the philological elements by which they are known to us. How far they may also have contributed to their ethnological elements is uncertain. Whence, indeed, the Hellenic stock is derived is still a problem scarcely yet attempted to be solved. Was it derived from Italy to Greece, as Dr. Latham inclines, not without reason, to believe (*Ethnol. of Europe*, p. 97), or from Greece to Italy? Was it the product of an intermixture of Keltic and Pelasgic blood, or of Pelasgo-Keltic and Semitic blood? Intermixture of blood, not purity of race, seems the law of highest development in the historic races; and hence, perhaps, it is that the old Keltic migration moved on westward and diffused itself over the great central area of Transalpine Europe through long unrecorded centuries, only making itself known by the shock with which it was rent in pieces when it came into collision with the younger historic races. Behind these Kelts came the Scytho-Sarmatian stock, still occupying to a great extent its original European area, though taking up so small and insignificant a section of the historic page; while the younger Germanic stock, Jacob-like, seizing the birthright and the portion of the elder, has overstepped it in the race, preoccupied the area of the displaced Kelts, shared in the spoils, and borne a prominent part in the reinvigoration of Southern Europe; and now entering on the possession of this vast continent of America, and of that other new world which lies sheltered in the temperate zone of the southern hemisphere, the Germanic—or as we too limitedly designate it, the Anglo-Saxon—race is entering on fresh aggressions and claiming a wider theatre for the arena of its triumphs. Whether the stirring among the Lithuanic and Slavonic races of Eastern Europe, which now thrills us with the rumours of war, and shakes all Europe with the coming struggle, be any symptom of the long dormant energies of her Scytho-Sarmatian stock awaking at length to assert the claims of a long-proscribed priority of birthright, is a question which had attracted the notice of Panslavic students of ethnology before it forced itself on the attention of European diplomatists.

On the Hyposulphites of the Organic Alkaloids. By Henry How, Professor of Chemistry and Natural History, King's College, Windsor, Nova Scotia.

In a recent communication to the Royal Society of Edinburgh,* I mentioned that when strychnine is exposed to the action of sulphide of ammonium, the hyposulphite of this base is formed, together with a peculiar and distinct product whose nature is not yet made out. The experiment affording these indications was made with free access of air, and I thought it extremely probable that the production of the hyposulphite was to be attributed in a great measure, if not entirely, to the formation in the first place of the hyposulphite of ammonia, from absorption of oxygen by the sulphide of ammonium, and the subsequent displacement of the volatile, by the fixed alkali, the transformation of the sulphur salt of ammonium being represented by the equation,

$$\underbrace{NH_4S, HS} + 4O = \underbrace{NH_4O, S_2O_2} + HO.$$

I reasoned that if the hyposulphite of strychnine really resulted from this succession of changes, the other alkaloids should present a similar deportment under the same circumstances. I, therefore, made corresponding experiments with some of these, and found that in the majority of the cases I tried, their hyposulphites are readily obtained; and they form

---

* Trans. Roy. Soc. Edin., vol. xxi., page 33.

so well-defined and beautiful a class of salts, as to merit a fuller and more accurate description than they have yet received. Indeed when I commenced their study I was of opinion that they were quite unknown, and it was only when my examination of this series of compounds was nearly completed, that I discovered that one of them, namely, the salt of quinine, had been already described. This description is accompanied by analytical numbers which, as I shall show in the sequel, must have related to a very equivocal specimen, as they are far from concordant with the real composition of the salt in question; and it is probable that the materials employed in its formation, by double decomposition, were not pure.

In addition to their great beauty and their mode of formation by a novel method, which is interesting in itself, these salts present claims for consideration on another ground. The peculiar nature of hyposulphurous acid renders its combinations with the alkaloids valuable as a means of establishing or controlling their atomic weight. Since this acid is instable in the free state, it is scarcely capable of forming acid salts, and basic compounds of the alkaloids being unknown, their hyposulphites must be composed in the relation of atom to atom of the proximate constituents. There are few subjects in organic chemistry which have been more discussed by various experimenters than the atomic weight of the vegetable bases, and most especially is this the case with quinine and cinchonine. Platinum salts of the alkaloids generally are now known not to afford by any means the infallible criterion they were once supposed to do; and a more certain indicator of the molecular equivalent, particularly of the natural alkalis, has been found in the amount of elements contained in their derived methyl, ethyl, and amyl bases. It is by this means that recent researches have placed it beyond doubt that quinine* and cinchonine† have respectively forty and thirty-eight atoms of carbon in their molecules. The hyposulphites of these bodies, as I shall describe them in this paper, are in complete accordance with these results.

As regards the production of the hyposulphites in general,

---

* Strecker, Comptes Rendus.

† Stahlschmidt. Annalen der Chemie und Pharmacie, vol. xc., page 218.

by this process, I have found that when the alkaloids are digested with fresh aqueous sulphide of ammonium, and some spirit of wine in an open flask, after a lapse of time, varying from a few hours to a day or two, hydrosulphuric acid cannot be detected, while hyposulphurous acid is present in abundance, in combination either with ammonia alone, or with it and the alkaloid employed. The comparative insolubility of the organic salt appears to be that which determines or favours its formation; for the deportment of all the bases is not the same in this process, which affords an interesting instance of the modifying influence exerted by circumstances over the play of chemical affinities; for here we see some of those alkaloids which are thrown down from their salts by aqueous ammonia, in their turn displacing this alkali when the circumstances are, as it were, reversed. It is also curious to observe how the presence of the fixed base determines the formation of hyposulphurous acid so rapidly in comparison with its production in aqueous sulphide of ammonium alone.

I have also found that some of the alkaloids dissolve when a current of sulphuretted hydrogen is passed through water in which they are suspended,* and these fluids yield hyposulphites by digestion. The salts of this acid may also be obtained by double decomposition in cases where the alkaloids afford sufficiently soluble and neutral compounds with other acids to start from, and I have used this method in several instances.

The following is the account of the salts I have examined, and I am again indebted to Professor Anderson, in whose laboratory in Glasgow this investigation was pursued, for some specimens of the pure alkaloids from his collection.

Hyposulphite of Quinine.—This salt is obtained after about a day's digestion of pure quinine with sulphide of ammonium and a little spirit of wine. It separates from the fluid in

---

* When strychnine is treated in this way, it yields a crystalline hydrosulphuret. The salt occurs in the form of colourless prismatic needles as deposited from cold water; it is very unstable, being resolved on standing into sulphuretted hydrogen, which escapes, and the pure base. This effect is brought about immediately on boiling the aqueous solution of the crystals. I am not aware that the hydrosulphuret of an organic base has been before observed.

opaque white tufts of needles, which are rendered pure by one crystallization from water. It is perfectly neutral to test paper, dissolves readily in boiling water, and is immediately deposited on cooling, as it requires about 300 parts of this menstruum at the ordinary temperature, to retain it in solution.

It is readily obtained by double decomposition between hyposulphite of soda and hot solution of neutral salts; but if the former reagent be added to a cold solution of the crystallized acid sulphate of quinine, the fluid becomes instantly milky, from the presence of precipitated sulphur, and smells of sulphurous acid; and when it has become clear, the walls of the vessel are seen to be covered with the peculiar dendritic crystals of the hyposulphite of quinine.

When dried, it afforded these results on analysis:—

3·883 grains, dried at 212°, gave
8·945 ... carbonic acid, and
2·365 ... water.
3·435 ... dried, gave by deflagration,
2·080 ... sulphate of baryta.

	Experiment.	Calculation.		
Carbon,	62·82	62·99	C_{40}	240
Hydrogen,	6·76	6·56	H_{25}	25
Nitrogen,	...	7·34	N_2	28
Oxygen,	...	14·72	O_7	56
Sulphur,	8·30	8·39	S_2	32
	100·00	100·00		381

which agree perfectly with the formula for the dry salt,

$$C_{40}\,H_{24}\,N_2\,O_4,\,HO\,S_2\,O_2$$

The crystals contain in addition two equivalents of water,

3·640 grains, air-dry, lost at 212°
0·170 ... water.

leading to a percentage of 4·67, and 4·51 is required by theory for the formula

$$C_{40}\,H_{24}\,N_2\,O_4,\,HO,\,S_2\,O_2+2\text{ aq.}$$

The mean results of the analyses of this substance by Wetherill,* to which I have already alluded, were these:—

---

* Liebig's Annalen, lxvi., page 150.

Carbon, . . .	61·35
Hydrogen, . .	6·72
Nitrogen, . .	8·30
Oxygen, . . .	15·13
Sulphur, . . .	8·50
	100·00

and the author concludes that quinine contains either 38 of 19 atoms of carbon, and 24 or 12 atoms of hydrogen, and the formula he calculated, $C_{38}\ H_{24}\ N_2\ O_4\ Ho\ S_2\ O_2$, agreed perfectly with his results. That which I have given, however, for quinine, is borne out by the researches of Strecker, before mentioned, and is now allowed to be the correct expression for the base.

Hyposulphite of Cinchonine is so readily obtained by double decomposition, owing to its sparing solubility, that I at once prepared by this means a sufficiency of the salt for analysis, though it is also formed by the other method. It is a very fine salt, crystallizing from water left at rest, in colourless, transparent, four-sided prisms, of large size. It dissolves in hot water with ease, but requires 205 parts of this menstruum when cold. It is perfectly neutral, and gave the following results on analysis :—

4·323 grains, dried at 212°, gave
10·300 ... carbonic acid, and
2·745 ... water.
4·860 ... dried at 212°, gave, by deflagration,
3·158 ... sulphate of baryta.

	Experiment.	Calculation.		
Carbon, . .	64·98	64·98	C_{38}	228
Hydrogen, .	7·05	6·55	H	23
Nitrogen, . .	...	7·97	N_2	28
Oxygen, . .	...	11·39	O_5	40
Sulphur, . .	8·91	9·11	S_2	32
	100·00	100·00		351

which agree with the formula

$$C_{38}\ H_{22}\ N_2\ O_2\ HO\ S_2\ O_2$$

The crystals contain one atom more of water.

4·985 grains, crystals, lost at 212°
0·120 ... water.

equal to 2·40 per cent., and 2·22 corresponds with the salt;

$$C_{38}\,H_{22}\,N_2,\,HO\,S_2\,O_2+aq.$$

This formula for cinchonine was arrived at by Stahlschmidt, as before mentioned, by acting upon the base with iodide of methyl. About the same time I had come to the same conclusion, from working with iodide of ethyl,* but ceased pursuing the subject on finding that I was forestalled. Having some of the iodide of the ethyl base, however, I tried to form the hyposulphite by double decomposition, but the former salt is so difficultly soluble in cold water as to crystallize out quite unchanged from the solutions of itself and hyposulphite of soda, mixed at the boiling-point. I had not a sufficient quantity of material to try any other process.

Hyposulphite of Morphia.—I was unable in two trials to obtain this salt by digestion of the base with sulphide of ammonium, hyposulphurous acid was formed, but remained in combination with ammonia alone. I was more successful by operating with concentrated hot solutions of hyposulphite of soda, and pure hydrochlorate of morphia. The fluid concreted to a solid mass, which, on being pressed when cold, and washed with a little cold water, was redissolved in the same liquid hot. It separated on cooling in white, silky, lustrous needles, very like the hydrochlorate. It was the pure hyposulphite. It is a comparatively soluble salt, requiring only 32 parts cold water for its solution; it is extremely soluble in this menstruum when boiling, less so in hot spirit, and so insoluble in the same at the ordinary temperature, that 1050 parts retain but one of the salt. It was quite neutral, and gave on analysis,

4·725 grains, dried at 212°, gave
9·755 ... carbonic acid, and
2·620 ... water
5·475 ... dried at 212°, gave
3·490 ... sulphate of baryta.

---

* I obtained an iodine salt, quite analogous to the product described by Stahlschmidt, crystallizing in fine 4-sided prisms; it gave 28·14 per cent. iodine, and 28·23 corresponds with the formula :—

$$C_{38}\,H_{22}\,N_2\,O_2\,,\,C_4\,H_5\,.\,I$$

which represents iodide of ethylocinchonine.

	Expt.	Calc.		
Carbon,	56·49	56·66	C_{34}	204
Hydrogen,	6·16	6·11	H_{22}	22
Nitrogen,	...	3·88	N	14
Oxygen,	...	24·47	O_{11}	88
Sulphur,	8·74	8·88	S_2	32
	100·00	100·00		360

These results show that the salt dried at this temperature retains water, and has the composition,

$$C_{34} H_{19} NO_6, HO, S_2 O_2 + 2 HO.$$

The crystals contain, in addition, two atoms of water;

5·08 grains, crystallized salt, lost at 212°,
0·25 ... water.

equal to 4·92 per cent., and 4·76 is required by this deduction of 2 aq. from the formula,

$$C_3 {}_4H_{19} NO_6, HO, S_2 O_2 + 4 HO.$$

Hyposulphite of Codeine.—This salt is readily procured by digesting the pure base with sulphide of ammonium. The fluid is evaporated to dryness after twenty-four hours, and the residue redissolved in a small quantity of hot water. The new salt then separates on cooling in rhombic prisms with dihedral summits; from dilute fluids these crystals may be obtained of large size. It is a soluble salt, requiring only 18 parts of cold water and very little spirit to take it up; it is neutral to test paper. It gave on analysis,

3·748 grains, dried at 212°, gave
8·309 ... carbonic acid, and
2·210 ... water

3·998 grains, dried at 212°, gave
2·662 ... sulphate of baryta.

	Expt.	Calc.		
Carbon,	60·46	60·67	C_{36}	216
Hydrogen,	6·55	6·17	H_{22}	22
Nitrogen,	...	3·93	N	14
Oxygen,	...	20·25	O_9	72
Sulphur,	9·12	8·98	S_2	32
	100·00	100·00		356

hence the formula of the salt, so dried, is

$$C_{36}\ H_{21}\ NO_6\ HO\ S_2\ O_2$$

the crystals contain in addition five atoms of water;

4·215 grains, crystalized salt, lost at 212°
0·457 ... water

the percentage resulting from this experiment is 10·84 : 11·22 is required to correspond with the formula;

$$C_{36}\ H_{21}\ NO_6\ HO\ S_2\ O_2 + 5\ aq.$$

Hyposulphite of Strychnine.—This salt is the principal product when the base is digested with sulphide of ammonium with free access of air. It is easily obtained by evaporation of the fluid after heating for a day or two, to complete dryness at 212°, and taking up the soluble portion of the residue in boiling water. The imperfectly examined product, elsewhere alluded to*, remains behind, and the fluid deposits the hyposulphite of strychnine on cooling, in colourless scales. By one other crystalization these may be obtained quite pure, and from a dilute solution I have seen them, even on the small scale, in rhomboidal plates with sides of one-eighth inch in length. It dissolves readily in boiling water, and of this liquid when cold, 114 parts retain but one of the salt. It is quite neutral and the following is its analysis,

4·211 grains,† dried at 212°, gave
9·740 ... carbonic acid,
2·195 ... water,

5 015 grains,† dried at 212°, gave
11·615 ... carbonic acid, and
2·737 ... water,

4·315 grains, dried at 212° gave
2·610 ... sulphate of baryta,

	Expt.		Calc.		
	I.	II.			
Carbon,	63·08	63·05	63·00	C_{42}	252
Hydrogen,	5·79	6·06	6·00	H_{24}	24
Nitrogen,	...	...	7·00	N_2	28
Oxygen,	...	...	16·00	O_8	64
Sulphur,	8·29	...	8·00	S_2	32
	100·00	100·00	100·00		400

---

* Trans. Royal Society of Edinb., vol. xxi., p. 33.

† I am indebted for these analyses to Mr Robert Davidson, a gentleman studying in Dr Anderson's laboratory.

whence it appears that the salt is not anhydrous at this temperature, but has the composition,

$$C_{42}H_{22}N_2O_4, HO, S_2O_2+HO.$$

and the crystals contain two atoms more of water,

$$\begin{cases} 4{\cdot}475 \text{ grains, air-dry, lost at } 212^\circ \\ 0{\cdot}175 \quad \ldots \quad \text{water,} \end{cases}$$

giving 3·91 per cent., and 4·30 agrees with this loss by the salt,

$$C_{42}H_{22}N_2O_4, HO, S_2O_2+3\text{ aq}.$$

Hyposulphite of Ethylostrychnine.—This salt cannot be obtained by the reciprocal action of the iodide of this base* and hyposulphite of soda, for, owing to the insolubility of the former in cold water, by far the greater part of it crystallizes out unchanged when the fluid cools. A small quantity of hyposulphite, however, is procured by evaporation of the mother liquor; it crystallizes in delicate needles, very soluble in water and spirit. The same compound may be obtained by passing a stream of sulphuretted hydrogen into the carbonate of thylostrychnine,† and allowing the liquid to stand exposed to a moderate heat. It is, however, in this case accompanied by a product which, to judge from appearances, is the same as that formed by the action of sulphide of ammonium upon strychnine, already more than once alluded to. This substance. which has a yellow colour, and is of extreme solubility in spirit, and nearly insoluble in water, seems to prevent the hyposulphite of ethylostrychnine, which is present in abundance, from being easily purified or readily taking on the crystalline condition. For this reason I was unable, with my stock of substance, to obtain the salt in a state suitable for analysis.

Hyposulphite of Brucine.—When brucine is digested with sulphide of ammonium and a little spirit, this salt is obtained in the course of a few hours. It crystallizes from the liquid, and requires but one other crystallization from boiling water, for complete purification. It then occurs in tufts of colourless prismatic needles, which are difficultly soluble in cold water,

---

* Trans. Royal Soc., Edin., vol. xxi., page 33

† Ibid., page 42.

105 parts retaining but one of the salt. It is perfectly neutral to test paper. In the analysis which follows, the salt was dried by simple exposure over oil of vitriol under a bell jar, as it decomposes in the water-bath, and only partially loses its water of crystallization *in vacuo*, and is, moreover, so hygroscopic in this state, as to absorb moisture with great rapidity when exposed to the air. The results it afforded were these:—

4·757 grains, dried over $HO\ SO_3$, gave
9·870 ... carbonic acid, and
2·810 ... water.
4·670 ... dried over $HO\ SO_3$, gave
2·220 ... sulphate of baryta.

	Experiment.	Calculation.		
Carbon, . .	56·58	56·67	C_{46}	276
Hyrogen, . .	6·58	6·36	H_{31}	31
Nitrogen, .	...	5·74	N_2	28
Oxygen, . .	...	24·66	O_{15}	120
Sulphur . .	6·53	6·57	S_2	32
	100·00	100·00		487

which accord in a perfect manner with the formula

$$C_{46}\ H_{26}\ N_2\ O_8\ , HO\ , S_2\ O_2 + 4\ HO.$$

The crystals contain another atom of water, which they lose over oil of vitriol.

9·765 grains, lost
0·175 ... water.

equal to 1·79 per cent., and 1·81 is required to make up the salt,

$$C_{46}\ H_{26}\ N_2\ O_8\ , HO\ \ S_2\ O_2 + 5\ HO.$$

When exposed to the temperature of 212°, the salt loses one-tenth of its weight in the course of time, and a portion of its sulphur evidently passes off in some form, for a specimen which had been heated to this point for about three days afforded less than 5 per cent. of sulphur on analysis. It was also found to be no longer soluble in boiling water, a considerable amount of a brown resinous matter remaining undissolved. The fluid contained some hyposulphurous and much sulphuric acid.

Hyposulphite of Papaverine.—I failed to obtain this salt in appreciable quantity by the digestion process. I ascertained however that it is a soluble salt.

Hyposulphite of Furfurine.—On addition of hyposulphite of soda to a solution of crystallized hydrochlorate of this base, an oil separates, which passes, after some time, into colourless needles.

Hyposulphite of Aniline may be formed by adding the soda salt to a strong solution of the neutral hydrochlorate of this volatile base, when it is speedily deposited in pearly scales. I could not obtain it pure, however, for it does not admit of re-crystallization. When taken up in warm water, in which it is readily soluble, the fluid becomes milky before the boiling point is reached; at this period aniline may be perceived to escape by its odour, and, immediately after, sulphurous acid is evolved in large quantity, and the salt is quite decomposed, the base not being a sufficiently powerful one to retain the hyposulphurous acid.

The following is a tabular view of the salts whose analyses are given in this paper:—

Hyposulphite of Quinine, dried at 212°,	$C_{40}\ H_{24}\ N_2\ O_4\ ,\ HO\ S_2\ O_2$
... ... crystallized,	$C_{40}\ H_{24}\ N_2\ O_4\ ,\ HO\ S_2\ O_2 + 2\ aq.$
... Cinchonine, dried at 212°,	$C_{38}\ H_{22}\ N_2\ O_2\ ,\ HO\ S_2\ O_2$
... ... crystallized,	$C_{38}\ H_{22}\ N_2\ O_2\ HO\ S_2\ O_2 + aq.$
... Morphia, dried at 212°,	$C_{34}\ H_{19}\ NO_6\ HO\ S_2\ O_2 + 2\ aq.$
... ... crystallized,	$C_{34}\ H_{19}\ NO_6\ HO\ S_2\ O_2 + 4\ aq.$
... Codeine, dried at 212°,	$C_{36}\ H_{21}\ NO_6\ HO\ S_2\ O_2$
... ... crystallized,	$C_{36}\ H_{21}\ NO_6\ HO\ S_2\ O_2 + 5\ aq.$
... Strychnine, dried at 212°,	$C_{42}\ H_{22}\ N_2\ O_4\ HO\ S_2\ O_2 + aq.$
... ... crystallized,	$C_{42}\ H_{22}\ N_2\ O_4\ HO\ S_2\ O_2 + 3\ aq.$
... Brucine, dried over SO_3,	$C_{46}\ H_{26}\ N_2\ O_8\ HO\ S_2\ O_2 + 4\ aq.$
... ... crystallized,	$C_{46}\ H_{26}\ N_2\ O_8\ HO\ S_2\ O_2 + 5\ aq.$

On some of the more recent Changes in the Area of the Irish Sea.

By the Rev. J. G. Cumming, M.A., F.G.S., Vice-Principal of King William's College, Castletown, Isle of Man.

In a memoir read before the Geological Section of the British Association, at its meeting in Cambridge in 1845, I directed attention to certain accumulations in the Isle of Man of boulder clay with post-pleiocene sands, capped by extensive terraces of drift gravel, and from an examination of the contents of these beds I endeavoured to trace out the general direction of the currents in the neighbouring seas at the period of their deposition. In the present paper I wish to point to a few facts bearing upon the subsequent removal of a large portion of them, and the formation of the basin now occupied by the Irish Sea.

I look upon the Isle of Man as affording, from its central position, an admirable clue to the changes which have taken place in this area, and as presenting to us a gauge by which to measure the relative level of the sea and land in the middle portion of the British Isles. For there is no evidence of any elevation or depression in more recent geological times affecting the Isle of Man *per se*, and not extending in a greater or less degree to the surrounding countries. All the evidences of later movements appear to be common to it and the surrounding coasts of Great Britain and Ireland.

I do not now enter into the question as to how the changes in the relative levels of sea and land were brought about, whether by the alternate elevation and depression of continents affecting the general level of the ocean, the change in intensity of gravitation at particular localities, or the absolute depression and elevation by volcanic or other agency of this portion of the globe. I have now simply to trace out certain facts indicative of considerable movements of an oscillatory character affecting the relative level of the sea and land, and to endeavour to point out those of the most recent date which have given their present contour to the shores surrounding the Irish Sea.

In various memoirs which I have read before the Geological Society of London during the last ten years, I have detailed the facts which lead me to the conclusion that during the deposit of the boulder-clay (*which was a period of depression*, and in which the climate of this region was of a more arctic character than is at this present time experienced), there was a gradual submergence of the Isle of Man, and (as I believe), of the coasts of the countries immediately around it to an extent of at least 1600 feet. At one period during the re-elevation (which was to an extent of about 15 feet above the present high-water-mark), there was a stationary interval, the sea-bed of the time of the formation of the great drift-gravel being left dry, and forming an extensive plain stretching out and uniting the present countries of England, Scotland, Ireland, and Wales.

I believe that at the same time England was similarly united to the Continent of Europe.

Then succeeded the second Elephantine period in which took place the immigration into these regions (amongst other quadrupeds now herein extinct), of the Cervus Megaceros or Great Irish Elk, whose remains have been found in the Isle of Man embedded in fresh water marls occupying basin-shaped depressions in the great drift-gravel plain.

The presence of these remains indicates the existence of *large treeless districts* during a considerable time in which the race greatly multiplied. Into the changes of climate and surface of the country which led to its ultimate extinction I will not now inquire. The basins containing the marls in which the remains are found, and the plains themselves, have since been covered with vegetation, and are still in many parts occupied by beds of turf, in which are found the trunks of trees, chiefly oak and elm.

But during the same period the ocean appears to have been quietly eating back its way into this terrace of the drift gravel, and resuming its more ancient sway, separating again Ireland and the Isle of Man from Great Britain, and cutting off the further immigration of animals and plants. Along all our coasts we find cliffs of this drift-gravel retiring in many places to a little distance inland, but where the gravel rests upon palæozoic rocks forming often part of the present coast-line.

It would be fruitless to speculate upon the length of that stationary period during which the process of the distruction of this upheaved sea-bed was going on. To excavate Castletown bay, in the south of the Isle of Man, alone must have occupied many hundred years. How many thousands must have been taken up in cutting out, by the same process and removing the materials between the southern extremity of the Isle of Man and a line extending from St David's Head to Carnsore point. How vain the attempt to measure the time.

That the destructive action was more rapid and intense from the south than the north, appears from the fact, that whilst in the north of the Isle of Man we have still remaining a tract of about fifty square miles of pleistocene deposits, in the south they are only preserved where resting upon the palæozoic rocks and at the head of deep bays. Why this should be the case we can immediately perceive by contrasting the narrow North Channel with the more open St George's Channel to the south.

One of the clearest proofs of the long-continued action of the sea, at a higher relative level than at present of about fifteen feet, is to be found in the Isle of Man along the south-eastern, southern and south-western coasts, in the presence of a series of water-worn caves, which are hardly reached by the highest tides which now occur. No one can inspect these coasts without observing the trace of extensive denudation and destruction above the present sea-line. The Eye of the Calf, the Burrough and Fistard Head, drilled completely through ; deep caves in the palæozoic rocks at Peel, Brada, Perwick, Langness, Santon, Port Soderic ; deep indentations in the drift-gravel wherever the sea wall of palæozoic rocks has been broken by a chasm, or descends below the line of high water. This is instanced in the horse-shoe bays and creeks of Port-Erin, Perwick, Port St Mary, Poolvash, Castletown, Derbyhaven, Coshnahawin, Saltric, Greenock, Douglas, Growdale, Laxey, and Cornah—all embraced by hard porpheries, basalts, schists, and carboniferous limestone, which are capped by the drift-gravel.

In most instances, these bays and creeks present, at their head or innermost recesses, perpendicular cliffs of the boulder-clay and drift-gravel, not rising in every instance from the present high water-mark, but from a level about fifteen feet above it, and having a low raised beach of a more recent date between them.

Of this lower raised beach I have now to speak. At the foot of certain slightly inland cliffs of the post-pleiocene period, on the coasts of the Isle of Man, England, Ireland, and Scotland, we have, extending down to the present high water-mark, and of various breadths, a low beach containing organic remains of the fauna now inhabiting our seas; at any rate, I am not aware of any extinct species being found in it as in the pleistocene beds.

The slope is generally gradual from the base of the pleistocene inland cliff to the present sea-level, and on it are situated the older parts of many of our sea-port towns.

Instances will probably occur to many here. The question is, does the present high water-mark really determine the extent of the elevation of the land since the formation of the cliffs in the pleistocene beds? I believe not. The elevation *must* at one time have been greater than it is at present; and it *may* have been to such an extent as a second time to lay dry a large portion of the area of the Irish sea. Why so?

We find on various parts of the coasts submerged forests. The growth of these forests we have good reason for attributing to a period posterior to the boulder-clay and drift-gravel, posterior to the formation of the inland cliffs in the pleistocene series. That they *must* have been so in *some* instances is certain; for, in the south of the Isle of Man, at Strandhall in Pooloash Bay, we find a submerged forest with the roots of the trees running down into the boulder-clay; the boulder-clay itself resting upon limestone-beds, grooved and scratched in direction N.E. and S.W. very nearly, and containing scratched boulders. As the drift-gravel was formed from the destruction of the boulder-clay, during the period of the re-elevation of the island, this at present submerged forest must also have grown after the formation of the drift-gravel terraces, and after the formation of the cliffs in it, and in the boulder-clay. In other words, it must have grown upon an area left dry by an elevation of the Irish Sea bottom, at an epoch subsequent to that long stationary period during which the sea eat back its way into that vast plain connecting the present British Isles, on which the Megaceros and other animals, which are now here extinct, lived and roamed.

The submergence of these forests points again to another subsidence of this area to the extent indicated by the present high-water mark. Whether it may have occurred, or been going on, during the historic period, will probably be a "vexata questio." It has been stated to me, on good authotity that, about forty years ago, after a violent storm which tore up large quantities of the submerged turf in Pooloash Bay, some remains of buildings were observed between high and low water. We venture to bring forward these few facts with the view of affording a clue to the formation of the present contour of the coasts of the Irish Sea, and of directing the attention of naturalists to the manner and period or periods in which occurred the immigration into the British isles of plants and animals, and also the manner in which the immigration may have been stopped, renewed, and stopped again.

On the Chemical Composition of some Norwegian Minerals.

By David Forbes, F.G.S., A.I.C.E.

During a residence of many years in Norway I have availed myself of the opportunity thereby afforded of studying the mineralogy of several districts of that country, with special reference to the circumstances under which the minerals occurred, and the causes which led to their appearance.

In order to do this with effect, I found it necessary to enter upon their chemical investigation, and it then became evident that, the occurrence in these minerals of elements so rare as to preclude chemists in general from studying their properties with that precision which has been the case with most of the other elementary bodies, involved the subject in much obscurity, and before I could have confidence in the results obtained, I was compelled to acquire some knowledge of the characters of several bodies which had not previously come under my observation, as, for example, thorina, yttria, tantalum,* columbium,*

---

* It must here be observed with reference to the names of tantalum and columbium that the original nomenclature has been strictly adhered to in this communication, tantalum being considered as the metal discovered by Ekeberg in 1802, in the Kimitotantalite, whilst columbium was previously discovered in 1801, by Hatchett, in the American columbite; this has since been called niobium by Rose.—*Vide Connell, London Philosophical Magazine*, 1854, p. 461.

glucina, zirconia, lanthanium, &c. It was only after I had familiarized myself as much as possible with these substances, that I proceeded to the analysis of a series of the Norwegian minerals which I had collected, paying especial attention to those containing the rare elements; and as many of the results obtained appeared likely to prove of interest, and some, apparently new species, were determined, it is proposed to communicate them from time to time.

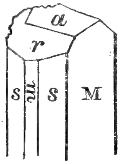
I.—EUXENITE.

This mineral was first found by Keilhau at Jolster in Nordre Bergenstift, and was recognised as a distinct species by Scheerer, who analysed it. Some time after a mineral was found by Weibye, near Arendal, supposed to be yttrotantalite, but on analysis by Scheerer (who states Tvedestrand as its locality), was proved to be euxenite.* When Mr Dahl and myself examined this district† we found two minerals pretty nearly agreeing with Scheerer's description in external appearance, but on further examination they were found to differ greatly from each other; one, however, found at Alve on Tromoen, an island near Arendal was evidently the euxenite of Scheerer.

This we found crystallized in prisms apparently belonging to the rhombic system, and well defined, but, from the faces being rough, and invariably covered by a thin greenish-gray scale, they could not be accurately measured.

The following measurements taken by Mr Dahl must, therefore, be considered only as approximative.

Fig. 1.



$s : M = 117°$ and $s : s = 126°$
$m : M = 90°$
$r : m = 154° 30'$
$a : r = 159° 30'$, or $140° 15'$
$a : M = 107°$
Also $a = P$, $r = m \overline{P} \infty$,

Where m is $\succeq 1$; $M = \infty \check{P} \infty$, $m \infty \overline{P} \infty$, $s = \infty P$.

Fracture conchoidal, with no trace of cleavage; colour, black;

---

[*] *Poggendorf Annalen*, vol. 50, p. 149.

[†] *Norske Magazin* for Naturvidenskab, viii., p. 3.

streak, reddish-brown. Lustre, brilliant and metallo-vitreous; translucent with a reddish-brown colour when in very thin splinters. Hardness, 6·5. Specific gravity taken at 60° F, of a small crystal = 4·99, and of a pure fragment of a large crystal = 4·89.

Heated in a glass-tube it does not change colour or lose lustre.

Before the blowpipe, infusible and unchanged. With borax in the oxidating flame it gives a brownish-yellow glass somewhat lighter in colour when cold. In the reducing flame it is unchanged, even on flaming. With phosphate of soda and ammonia it gives a glass which is greenish-yellow whilst hot, but nearly colourless on cooling. Gives no reaction either of titanium or manganese, although it contains both these metals.

The analysis was conducted as follows :—

20·81 grains of the pure mineral impalpably powdered were ignited in a gold crucible and lost 0·60 grains, becoming somewhat lighter in colour. 160 grains bisulphate of potash were then added, gradually fused, and kept melted for several hours until the mineral appeared completely decomposed. As much as possible was removed from the crucible whilst in a pasty state, softened with cold water in an agate mortar, and reduced carefully to fine powder; the crucible was likewise washed with cold water, and the whole being made up to 16 oz., was allowed to digest for 18 hours at the ordinary temperature in a beaker.

The clear supernatant fluid was carefully decanted off; 16 oz. more cold water added and allowed again to digest for 24 hours; this was repeated a third time and the insoluble matter then thrown on to a filter, well washed with cold water, dried, and incinerated. It weighed 8·03 grains. This residue on heating became of a brilliant yellow colour, but was quite white when cold, and possessed all the characters of columbic acid.*

The solution was now boiled for some time, when a precipitate fell, which was washed, dried, and incinerated, and

---

* As to whether the columbic acid might contain also tantalic acid I am not prepared to say, as I believe there is not at present any accurate means of separating these two substances.

weighed 2·99 grains. It gave the reaction of titanic acid before blowpipe, but evidently contained columbic acid, as it became bright yellow on ignition, while it was of the usual colour when cold. I am not acquainted with any means of separating these acids completely.

The solution was now precipitated by ammonia, and the precipitate filtered off and carefully washed. In the filtrate a small amount of lime and magnesia were respectively determined by oxalate of ammonia and phosphate of ammonia.

The precipitate itself was dissolved in hydrochloric acid, rendered as nearly neutral as possible by ammonia, and precipitated by oxalate of ammonia—a white precipitate fell which was collected and washed. The washings at first went milky through the filter, but it was found that it could be prevented by adding a few drops of oxalate of ammonia to the wash water.

This precipitate was ignited, dissolved in hydrochloric acid, and the cerium separated by sulphate of potash, filtered off and determined as usual. The yttria was precipitated by ammonia from the solution and dried. It then weighed 9·24 grains, but as it did not look well I re-dissolved it and again precipitated, when it was found to weigh only 6·11 grains, so that the first precipitate was evidently a basic salt.

The filtrate from the precipitation of the oxalates was now precipitated by hydrosulphate of ammonia,—and this precipitate after solution in nitrohydrochloric acid was treated with potash to separate alumina, and the uranium afterwards separated from the iron by carbonate of ammonia.

The following results were thus obtained :—

	Grains.
Employed in analysis,	20·81
Loss on ignition—reckoned as water,	0·60
Columbic acid,	8·03
Titanic acid (with some do.),	2·99
Carbonate of lime,	·51
Phosphate of magnesia,	·11
Alumina,	·65
Sesquioxide of iron,	·46
Oxide of uranium,	1·13
Yttria,	6·11
Sesquioxide of cerium,	·73

Which, when tabulated will stand as follow :—

	In 20·81.	In 100.		Oxygen.
Columbic acid, .	8·03	38·58	52·94	?
Titanic acid, with some columbic acid, .	2·99	14·36		5·79
Alumina, .	·65	3·12		1·45
Lime, . .	·28	1·37		0·38
Magnesia, .	·04	0·19		0·07
Yttria, . .	6·11	29·36		·12
Protoxide of cerium,	·68	3·31		0·47
Protoxide of iron, .	·41	1·98		0·43
Protoxide of uranium,	1·08	5·22		0·61
Water, . .	·60	2·88		2·56
	20·87	100·37		

As we have no fixed atomic equivalent for either columbium or yttrium, we cannot calculate the amount of oxygen, but taking them at the old numbers of 180 and 32, the oxygen will be 4·53 in the columbic acid, and 5·87, in the yttria, which will make the relation of the amounts of oxygen as 10·32 in the acids to 10·73 in the bases ; but it is useless attempting to deduce a formula from this analysis until we have more information as to the composition and atomic equivalent of columbic acid and yttria.

For the sake of comparison Scheerer's result is annexed :—

	From Jolster. Sp. Gr. 4·60.	From Arendal. Sp. Gr. 4·73 to 4·76.
Metallic acids, .	57·60	53·64
Yttria, . .	25·09	28·97
Protox. of uranium,	6·34	7·58
Protox. of cerium,	3·14	2·91
Protox. of iron, .	—	2·60
Lime, . .	2·47	—
Magnesia, .	0·29	—
Water, . .	3·97	4·04
	98·90	99·74

When comparing the mineral here analysed with that from Arendal by Scheerer we find that the sum of the metallic acids, and the yttria, agree, but that Scheerer has no alumina, lime, or magnesia, and considerably more water and protoxide of uranium than I have found.

II.—TYRITE.

The other mineral which in external appearances might be confounded with Euxenite, was found on the same island by Mr Dahl, at a place called Hampemÿr, and was crystallized in prisms, having a quadratic section, but too irregular and unreflecting to admit of measurement, and one apparently belonging to the tetragonal system. Fracture conchoidal; and no trace of cleavage apparent; exceedingly brittle; hardness 6·5. The specific gravity of a crystal was 5·30, and of a massive piece was 5·56 at 60° Fahrenheit. It scolour and lustre were perfectly the same as those of Euxenite, and it is translucent in thin splinters.

When heated in a glass tube it decrepitates strongly, evolves water, and the powder resulting from its decrepitation is of a brilliant yellow colour.

Before the blow-pipe it is soluble in borax to a glass of a reddish yellow colour when warm, but colourless on cooling. In phosphate of soda and ammonia it is soluble with difficulty, and appears to leave portions undissolved. The glass is greenish yellow whilst hot, and green when cold.

The analysis was conducted as follows :—

A portion, in fine powder, weighing 29·42 grains, was cautiously heated to redness, when it became of a greyish yellow colour, and the loss was estimated as water.

Another portion of mineral finely pulverized was digested with pure concentrated sulphuric acid in a platinum vessel for a considerable time; it appeared to decompose easily and completely, leaving a white powder, which was several times successively digested with sulphuric acid well worked with water, and weighed.

This substance reacted as columbic acid, was of a pure white colour when cold, but became intensely yellow when heated, recovering, however, its original colour completely on cooling. It was readily soluble in hydrofluoric acid, and this solution deposited stellar groups of crystals on concentration.

The solution was then precipitated by ammonia, filtered, and lime determined in the filtrate by means of oxalate of ammonia; no magnesia was found to be present.

The precipitate on filter was dissolved in a little dilute sulphuric acid, then greatly diluted with water, and boiled for some time, when a small quantity (0·26 gr.) of a white precipitate fell, which was weighed and tested for titanic acid, but found only to consist of columbic acid, (that is, if no tantalum is present, as no means of separating them is known,) it was therefore added to the former.

The solution after this precipitation was supersaturated with ammonia in excess, and then oxalic acid added until a very faint acid reaction was perceptible. The oxalates thus precipitated were filtered off, washed, ignited, and dissolved in hydrochloric acid; the solution treated with sulphate of potash to separate the cerium and yttria, which were both determined in the usual manner.

The filtrate from the precipitated oxalates was now precipitated by hydrosulphuret of ammonia, and the alumina, iron, and uranium determined as in the previous analysis.

The results obtained were as follows :—

Mineral employed for water determination,	29·42	grs.
Loss on ignition, . .	1·33	,,
Mineral employed in analysis, .	12·35	,,
Columbic acid obtained, . .	5·29	,,
Do. on boiling, . . .	0·26	,,
Carbonate of lime, . .	0·18	,,
Ignited oxalates, . .	4·37	,,
Yttria, . . .	3·67	,,
Sesquioxide of cerium, . .	0·71	,,
—— of iron, . .	0·86	,,
Oxide of uranium, . .	0·39	,,
Alumina, . . .	0·71	,,

From which the per centage calculated will be :—

		Oxygen.
Columbic acid, .	44·90	?
Alumina, .	5·66	2·64
Lime, .	0·81	0·23
Yttria, .	29.72	?
Protoxide of cerium,	5·35	0·77
—— of uranium,	3·03	0·35
—— of iron,	6·26	1·38
Water, .	4·52	4·02
	100·25	

If we calculate the yttria and columbic acid as before, the ratio between the acids and bases are as 5·28 to 11·31, which is most likely as 2 to 1, although it is impossible, as in the case of Euxenite, to deduce any satisfactory formula before we are better acquainted with the compounds in question.

From this analysis the mineral appears to be a new species, and it has accordingly been called Tyrite.* It can be most readily distinguished from the Euxenite by the following characters:—

By its specific gravity much higher than that of Euxenite, and by being brittle. By its behaviour when heated. By its reactions with phosphate of soda and ammonia.

And, lastly, by its chemical composition, and the absence of titanic acid. In short, though it is impossible at present to fix a formula for it, it must be regarded as mainly consisting of a hydrous columbate of yttria.

III.—YTTROTITANITE OR KEILHAUITE.

This mineral was first found by Weibye, at Buöen, an island, near Arendal, and was analysed simultaneously by Scheerer and Erdmann, who respectively named it yttrotitanite and keilhauite—it was not crystalized although it possessed two cleavages. Mr Dahl has however found it crystallized at Arkerœn, in regular and distinct crystals, belonging to the monoklinohedric system. Some of these weighed as much as 2½ lbs., and from their size and rough surfaces could only be measured by the hand goniometer.

The following figures give the crystalline forms :—

Fig. 3.

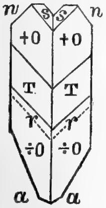


Fig. 2.

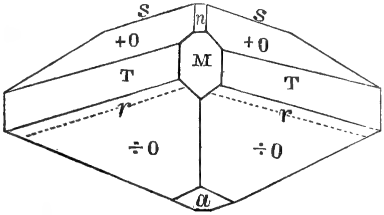
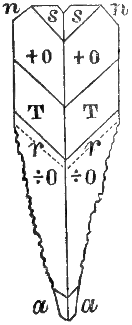


Fig. 4.



---

* From Tyr, the Norwegian god of war, this mineral being discovered at about the same time as the commencement of the present war.

From which form approximative measurements were obtained by the common goniometer.

M : T = 147°
+ *o* : *s* = 149°
M : *n* = 125°
a : M = 122°
− *o* : T = 153°30′
− *o* : *a* = 143°30′

The angle M : T is the average of measurement from four different crystals. As the junction of T : T is always cut off by the plane M, and all the crystals hitherto obtained are hemitropes, so that the planes T : T, which form an acute angle, belong each to its own half of the crystal, the direct determination of the angle T : T is too uncertain, but reckoned from M : T = 147°, it will be 114°. The angle *a* = 58°.

From these data Mr Hansteen has had the kindness to calculate the following values :—

Axes *a* : *b* : *c* = 0.835 : 1 : 0·766, and

a : S = 140°, 42′
S : +*o* = 149°, 14′
+*o* : T = 135°, 11′, 17″
T : *o* = 151°, 18′, 43″
− *o* : *o*P = 143°, 34′

720°

M : *n* = 123°, 27′
a : T = 114°, 25′, 43″

The observed forms are—

a = *o* P
+ *o* = + P
÷ *o* = ÷ P
r = 2 P
s = + $\frac{1}{2}$ P
n = P ∞
M = ∞ P ∞

The positive terminal planes have on most of the crystals a strong vitreous lustre, as if polished by friction, and have a great number of small furrows arranged in rows parallel to the edges between T and + *o*. The vertical prismatic planes are smooth, but possess much less lustre. The negative terminal planes are rough, and irregular by reason of an oscillating combination between the planes − *o* and T, by which the crystals sometimes are lengthened in this direction, as shown by Fig. 4. The cleavage planes are very distinct, and are parallel to the plane *r*; and it was easy to cleave out pieces of a rhombic section, the angle being about 138°, no third cleavage was observed.

The specific gravity at 60° Fahr. was found to be 5·53. In analysing it I determined to follow the method employed by Scheerer, and in consequence 22·78 grains were digested in hydrochloric acid, by which it was readily decomposed. Much water was then added, and the whole filtered from the silica; but I found that on attempting to precipitate the titanic acid from this solution by boiling, as stated by Scheerer, no precipitation occurred; the solution was therefore thrown down by ammonia, washed, and re-dissolved in dilute sulphuric acid, when a small quantity of silica remained undissolved, which was filtered off, and much water then added to this solution, and boiled when the titanic acid was readily thrown down and weighed. On ignition it was found to be slightly tinged with iron.

In the filtrate from the ammoniacal precipitate, lime was precipitated by oxalic acid; the oxalate collected, ignited, and dissolved in acetic acid, to separate some manganese which was precipitated with it, and determined as sulphate. The solution from this gave a precipitate of oxalate of lime. And on evaporation of the filtrate a small quantity of insoluble matter remained which reacted for titanic acid before blow-pipe, and was considered as such.

The silica was treated with hydrofluoric acid, which left a very small quantity of titanic acid undissolved.

The solution from which the titanic acid had been separated by boiling was now neutralized by ammonia, and precipitated by oxalate of ammonia, and the yttria determined in this as usual.

The filtrate from this last precipitate was found to contain iron, alumina, and glucina, which, after precipitation by hydrosulphuret of ammonia, were all determined in the usual way; the glucina being separated by carbonate of ammonia, and the sesquioxide of iron freed from alumina by caustic potash. The results obtained were:—

Mineral employed,	22·78	grs.
Impure Silica obtained,	7·66	...
Titanic acid, from do.,	·59	...
Titanic acid, from solution,	·32	...
Titanic acid, with trace of iron,	6·39	...
Silica, from ammoniacal precipitate,	0·07	...
Yttria,	1·09	...
Sulphate of lime,	10·80	...
Sesquioxide of iron,	1·63	...
Alumina,	1·83	...
Glucina,	0·12	...
Oxide of manganese,	0·07	...

From which the following percentage results will be obtained:—

	In 22·78.	In 100.	Oxygen.		
Silica,	7·14	31·33	15·06	26·24	
Titanic acid,	6·39	28·84	11·18		
Alumina,	1·83	8·03	3·75	4·07	12·16
Glucina,	·12	·52	·32		
ime,	4·45	19·56	5·56	8·09	
Yttria,	1·09	4·78	·95		
Protoxide of iron,	1·56	6·87	1·52		
... manganese,	·06	·28	·06		
	22·64	99·41			

This analysis agrees pretty well with those of Erdmann and Scheerer, with the exception that the yttria is not more than half the amount found by them, and the lime and alumina are both somewhat higher. Erdmann gives the formula, as

$$3\dot{Ca}^3\,\dddot{Si}^2 + \ddot{\bar{R}}\,\dddot{Si} + \dot{Y}\,\ddot{T}^3$$

which, however, does not seem to be correct, as he supposes the titanic acid to be entirely combined with the yttria, which here is evidently not the case. It seems probable to me that the yttria only replaces a part of the lime, and although it may be an essential ingredient, in so far as it may never be absent, still it most probably does not play so important part as to show itself in the formula. Supposing the titanic acid to play the part of a base, we shall find that the oxygen of the base to that of the silica will not be far from the ratio of 3 : 2, not farther indeed than the several analyses differ amongst themselves. This will, therefore, be the same as Sphene, so that the formula might be considered as

$$\overset{\cdots}{\not{\mathrm{R}}}\,\overset{\cdots}{\mathrm{Si}}^{\frac{2}{3}} \text{ or } (\dot{\mathrm{R}}^{3}\,\overset{\cdots}{\not{\mathrm{R}}})\,\overset{\cdots}{\mathrm{Si}}^{\frac{2}{3}}$$

as the percentage of silica is the same as in Sphene. It appears to me doubtful whether we can consider any mineral as true silico-titanates or silico-tantalates; and it is probably preferable not to do so, on account of the great differences in properties between silicic and titanic acids.

On Mineral Charcoal. By ROBERT HARKNESS, F.R.S.E., F.G.S., Professor of Mineralogy and Geology, Queen's College, Cork.

Mineral Charcoal, or, as it is termed in some parts of England, " Mother Coal," occurs in greater or less abundance in almost every description of coal.* It usually presents itself in the form of a black, pulverulent, fibrous, silky-looking substance, coating or embedded in the ordinary mass of the coal. Sometimes, however, instead of having a fibrous structure, it is somewhat granular, and both these forms may, in some cases, be seen in the same coal.

This substance, when fibrous, makes its appearance in a shred-like state, but when it has a granular aspect it is frequently manifest as a thin layer covering a face of the coal, and these layers often form laminæ among the seams of coal. The occurrence of mineral charcoal in fossil fuel is not a circumstance which always prevails, and there are certain conditions connected with coal-seams which lead to the prevalence, in some beds of coal, of this matter, while in others it makes its appearance only to a very slight extent.

On seeing a portion of mineral charcoal embedded in a mass of ordinary coal, it will be at once perceived that it must have owed its occurrence, in such a situation, to the influence of causes which have not operated uniformly on coal-seams; and when we see a considerable mass of this substance associated

* The substance here termed mineral charcoal is not, in its chemical composition, in all cases allied to anthracite, but is that matter which, in its external aspect, somewhat resembles wood charcoal, and to which the name mineral charcoal has been applied by mineralogists.

together, it will easily be perceived that this association is the result of partial drifting, since we have mineral charcoal so combined that, although each separate piece has its fibres parallel, the whole of the pieces are confusedly heaped together.

This partial drifting of the matter which now occurs in the form of mineral charcoal is borne out by other circumstances, which at once show that this substance must, to a certain extent, be regarded as an accidental feature in coal. Among these circumstances, we find the evidence afforded by the interculated strata is such as to justify the conclusion that when this mineral charcoal makes its appearance in considerable masses in a fibrous state, it owes its position to partial drifting.

In the sections given by Mr Dawson of the coal-measures of South Joggins, Nova Scotia (*Quart. Jour. Geol. Soc.*, Vol. X., p. 3), we have two instances given of the occurrence of mineral charcoal, and in both of these the nature of the accompanying deposits is such as to indicate the operation of drifting causes.

In the first instance, we have a "coal, with much mineral charcoal," 8 inches thick, lying upon "under-clay, hard and *arenaceous*," 3 feet in thickness, a description of floor which shows considerable motion in the water from whence it emanated. The second instance furnishes us with "coal and bituminous shale, prostrate trunks of trees, and mineral charcoal," half-an-inch in thickness, resting on "sandstone with clay partings," also indicating the prevalence of motion during the deposition of this bed containing mineral charcoal.

The coal-fields of Great Britain, likewise, provide us with proofs that this matter also occurs among the coal in consequence of partial drifting. As an instance of this, in two coal-seams which are wrought near Sanquhar, in Dumfriesshire, where the great coal-field of Scotland has its most southerly limit, we meet with the same causes influencing the appearance of mineral charcoal.

Here we have a coal called the Calmstone-seam, from the circumstance that its roof is formed of fine indurated light-grey clay, a deposit which must have sprung from a comparatively tranquil medium, and in this coal we have few traces of the mineral charcoal, the coal having a bright aspect. In the other coal, which is known under the name of the Creepy, we have abundance of this substance, more particularly in the higher part of the seam, which in some spots is absolutely composed of mineral charcoal. The nature of the deposits overlying this bed of coal points out from what circumstances it derived its peculiar composition. The roof of the Creepy-coal consists of a flaggy sandstone, such as would arise from the operation of water in motion, in the form of currents; and previous to the deposition of this sandstone roof these currents carried portions of plants, which became water-logged and fell to the bottom, forming the mineral charcoal which enters so largely into the composition of the Creepy-coal.

The occurrence of mineral charcoal is not confined to the coal of the carboniferous formations alone. The oolitic coal of Virginia also affords this matter, and the tertiary coals of Great Britain, as these are developed at Bovey Tracy, also furnish us with mineral charcoal.

These, however, differ in their nature, and likewise in their aspect, from those which are obtained from the true coal-fields of Great Britain, yet there is every reason to conclude that they originated from the same conditions.

As regards the nature and origin of mineral charcoal, the appearance which this substance presents at once furnishes sufficient proof of its being vegetable matter. However, as it has both a granular and a fibrous aspect, so it seems to differ in its vegetable nature. When submitted to the microscope, the granular variety does not afford the same regular structure as does the fibrous kind. The former appears to consist of a mass of cells which are comparatively only slightly elongated, and these have, so far as can be seen, the structure of simple cellular tissue, which has probably been derived from the ordinary plants usually entering into the composition of coal. When this tissue is sufficiently hardened to admit of its being sliced transversely, an arrangement of cells in a hexagonal form is manifest, a description of tissue which occurs in the woody cylinder of sigillaria as well as in the gymnospermous vegetation which makes its appearance in the carboniferous formation.

Concerning the more fibrous variety of mineral charcoal, this exhibits, not only when viewed under ordinary circumstances, but likewise when submitted to microscopical examination, a more highly organized structure than that which exists in the granular kind. A longitudinal section of the fibrous variety shows that the walls of the cells, instead of being simple, are marked by numerous hollow spaces which have commonly an elliptical form, the major axis of the ellipsis being across the cells. The spaces are closely approximated one to another, and they present a form of tissue which is allied to the discigerous tissue of conifera, and the fibrous mineral charcoal appears to have been derived from the woody portion of plants which had some affinity to this tribe of gymnosperms.

On comparing this fibrous tissue with that of a fossil obtained from the coal mines at Ince Hall, near Wigan, which I procured last summer when visiting this neighbourhood along with Mr Binney, I find that the structure of both these is such as to support the conclusion that the fibrous mineral charcoal has been derived from plants of a similar character with the fossil referred to. This fossil, which seems to belong to the Calamodendron of Brongniart is, in part, converted into iron-pyrites, and in part into mineral charcoal. It possesses the markings of nodi, such as prevail in calamites, and these are about half an inch separate from each other. But as the specimen is devoid of the external portion, its affinity to the ordinary plants of the carboniferous formation cannot be distinctly made out. There is sufficient evidence to show that this plant has, however, been the fertile source of the fibrous mineral charcoal. From the structure of this variety of mineral charcoal, and from the nature of the vegetation from which it appears to have been principally derived, it would seem that forms somewhat allied to conifera were the tribe of plants supplying this substance.

As mineral charcoal occurs abundantly in some varieties of coal, it must have been derived from plants which prevailed to a considerable extent during the coal epoch ; and since neither Sigillaria, the most prevalent form, nor Lepedodendron afford the discigerous structure which manifests itself in mineral charcoal, this woody matter may have formed portions of another prevailing genus, viz., calamites, the nature of the tissue of which we are still in ignorance. Lindley and Hutton, in the Fossil Flora, observe, when describing calamites nodosus (plate 15, 16), "This belongs to a large and well-known class of fossils of which the stems are more abundant in the beds of the carboniferous formation of the North of England than any others. They are often found in close alliance with the coal itself, *especially when thin layers of mineral charcoal are discovered upon it:*" a circumstance supporting the conclusion of the relation of mineral charcoal to calamites, and when it is considered that the specimen of calamodendron from Wigan, containing the same form of structure, is marked by nodi, one of the characteristic features of the calamites, the conclusion that this substance has been derived from such plants, is, to some extent, borne out, leading to the inference that calamites were gymnospermous plants having some affinity to the modern conifera in their internal structure; which probably may have consisted of a narrow woody cylinder, marked with discs, enveloped in a great mass of simple cellular matter.

With respect to the mineral charcoal which is found in coaly deposits of an age posterior to the carboniferous formation, this also partakes of a gymnospermous character. Among the oolitic coal of Virginia, North America, mineral charcoal of this nature occurs; but, according to Mr Darker, this belongs rather to cycadia than conifera. That which is met with in the lignite of Bovey Tracy is decidedly coniferous, and the mode in which the circular areolœ arrange themselves, as seen in longitudinal section, shows an intimate relation to some of the modern conifera. In these three varieties of coal we have three forms of fibrous mineral charcoal, which are, to a considerable extent, related to each other, and which have been derived from representatives of the same tribe of plants, viz., gymnosperms, and the mode in which these are met with at once points out that the partial drifting of vegetable matter was the cause of the occurrence of mineral charcoal.

On a Simple Variation Compass. By William Swan, F.R.S.E., F.R.S.S.A.*

About two years ago, my friend Mr John Adie communicated to the Royal Scottish Society of Arts the description of a new variation compass. His instrument, which is intended to be used along with an ordinary theodolite, was devised for the purpose of ascertaining the magnetic meridian with greater accuracy than is attainable, either by the use of the compass usually attached to theodolites, or by employing the more ordinary forms of the azimuth compass.

Mr Adie's very elegant invention is described in the Transactions of the Royal Scottish Society of Arts, vol. iv., p. 138. It consists of a delicately-suspended compass-needle, inclosed in a tube furnished with collars, which are placed in the Ys of the theodolite, the telescope having been previously removed. The ends of the needle, which are brought to fine points, are nearly in contact with finely divided glass diaphragms; and the needle being viewed through the diaphragms by powerful eye-pieces, has its ends accurately referred to those divisions. It is easy to see how, in this manner, the axis of the tube with its collars,—which, when placed in the Ys, is coincident with the axis of the theodolite telescope occupying that situation,—can be placed parallel to the axis of the needle; and the reading on the horizontal limb of the theodolite corresponding to magnetic north may be obtained.

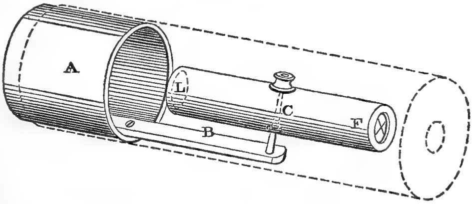
From actual trial, I was so much satisfied of the excellence and utility of Mr Adie's instrument, that I felt desirous of having something of the same kind applied to a Kater's altitude and azimuth circle in my possession; but as the telescope of that instrument, unlike that of the ordinary theodolite, does not admit of being *removed*, I was obliged to adopt an arrangement totally different from Mr Adie's.

The instrument I devised was constructed for me by Mr Adie in the autumn of 1852; and I now describe it, in the hope that it may be useful to persons who, possessing instru-

---

* Read before the Royal Scottish Society of Arts.

ments analogous to Kater's circle, or indeed any form of theodolite, may wish to make observations of magnetic declination.



It consists of a collar A, fitted so as to slide without much friction upon the object-end of the telescope of the theodolite with which it is to be used; an arm B, projecting in front of the telescope, furnished with a fine steel point C; and a small collimating magnet LF, supported on an agate cap, which turns on the point C. The best form for the collimating magnet, would, I conceive, be that of a hollow steel cylinder, carrying at one end a lens L, and at the other a cross of spider-lines F, as represented in the figure,—a construction which has been adopted in various magnetic declinometers. In the instrument made for me by Mr Adie, instead of the cylinder shown in the figure, there are two steel plates, each 5 inches long, 0·3 broad, and 0·02 thick, placed parallel to each other, and connected at the ends by light frames of brass; an arrangement which answers exceedingly well. One of these frames carries the lens L, and immediately behind the other, and between the plates, so as to be out of risk of injury, is placed a diaphragm carrying the cross fibres F. The lens is not achromatic, but as its aperture is only 0·2 inch, while its focal length is 4·7 inches, the image of the cross fibres formed by it is tolerably well defined. I should recommend, however, the adoption of an achromatic lens of greater aperture, and shorter focal length than that which I have described, and the hollow cylindrical magnet instead of the parallel steel plates; for the cylindrical magnet will admit of the lens and cross lines of the collimator being more firmly fixed in their places, while at the same time they will be less liable to derangement from handling the magnet. It is scarcely necessary to explain, that the rays of light proceeding from the cross fibres, which are placed in the principal focus of the lens, are rendered parallel by the lens, and thus enter the telescope of the theodolite in a fit state to be brought to focus at the diaphragm wires, where they form a distinct image of the cross fibres. A light tube represented in the figure by dotted lines, slides over the whole, so as to protect the magnet from currents of air; and is furnished with an aperture at its end, covered with glass, through which light is thrown by a small reflector to illuminate the cross fibres.

The method of observation consists in first making the image of the intersection of the collimator cross fibres coincide with the middle diaphragm wire of the theodolite telescope, which is easily effected by means of the tangent screws of that instrument, and then reading off the verniers on its horizontal limb. If the magnetic axis of the magnet were parallel to the optical axis of the collimator, the reading on the limb for magnetic north or south would thus be at once obtained; but as such a condition can never be strictly fulfilled in practice, it is necessary, where an accurate result is wanted, to repeat the observation with the needle in an inverted position. For that purpose the agate cap is made to screw into opposite sides of the magnet, which thus admits of being suspended with either side uppermost. By taking the mean of the readings in the two positions of the magnet, any error caused by want of parallelism in the line of collimation and the magnetic axis will be either wholly or nearly eliminated. Half the difference of the readings in the two positions of the needle, carefully determined from a number of observations, may be registered and applied as an index error, when the needle has been observed without having been inverted; and such a mode of observation will probably be sufficiently accurate for the ordinary purposes of the surveyor.

It is desirable that the instrument be adjusted so that the difference of the readings in the two positions of the magnet may not be *great*. For as the correction, for want of perfect adjustment, obtained by taking the mean of those readings, will generally be only approximate, it is well that any residual error should be confined within as narrow limits as possible.

In order to ascertain the variation of the compass, or to apply the observations of the magnet to the ordinary purposes of surveying, it is necessary to direct the theodolite telescope to a meridian mark, or other proper object, and to read off its horizontal limb; and it is desirable that this should be done both before and after observing the magnet. The collar A should be adjusted to the telescope before taking the observations of the meridian mark; and the magnet and its cover should be put in their places, and removed again, with as delicate manipulation as possible, in order to avoid disturbing the theodolite,—the cover for that purpose being made to slide off and on with very little friction. Practically, I have found no sensible discrepancy in the readings for the meridian mark arising from disturbances caused by handling the magnet and its cover; but if it be deemed desirable to avoid altogether the chance of such errors, it may be done by furnishing the aperture in the cover, which illuminates the collimator cross, with a piece of parallel plate-glass. The meridian mark may then be seen through this glass, and observed without removing the cover, immediately after observing the magnet. Any error due to refraction will be eliminated by reversing the cover, when it is replaced after reversing the magnet, and again observing the meridian mark; but a good piece of glass, such as that which is used for making the mirrors of sextants, will cause no error from refraction appreciable with the magnifying power of an ordinary theodolite telescope.

It is always proper, however, to reverse the cover, in order to eliminate the effects of any attraction it may exert on the magnet; and for the same purpose, I have always observed with the vertical limb of my Kater's circle facing alternately east and west. I may add, with reference to the observations of magnetic declination given in the sequel, that I have since ascertained that when that instrument was brought as near as was possible to a collimating magnet, suspended by a very delicate silk thread, and observed through a telescope, it caused no perceptible deflection.

The practical limit to the accuracy of observations made by such an instrument as that which I have described, is the friction of the point of suspension. When the needle shows any symptom of not swinging freely, the point should be carefully sharpened on a hone,—a process which any one may learn to perform for himself.

I find that the most consistent readings are obtained, not by waiting until the magnet comes to rest, but by causing the theodolite wire to bisect the arc of vibration of the magnet, by estimation, as soon as that arc is reduced to about 8′ or 10′. If the magnet has come to rest, it is easy to make it vibrate again in a small arc, by cautiously approaching to it a magnet or a piece of iron, which is again removed to a sufficient distance before making the observation.

As an example of the performance of the instrument, I select the last observation of magnetic declination I have made.*

Greenwich Mean Time.	Observed Azimuth of line of Collimation of Magnet: mean of two Verniers.
1854, April.	Before reversal of magnet.
$10^{\mathrm{d}}\ 1^{\mathrm{h}}\ 30^{\mathrm{m}}$	77° 46′ 10″
34	46 10
38	46 5
	After reversal of magnet.
$1^{\mathrm{h}}\ 45^{\mathrm{m}}$	77° 43′ 37″
48	43 45
51	42 25
Mean of all, $1^{\mathrm{h}}\ 41^{\mathrm{m}}$	77° 44′ 42″

Azimuth of the magnetic axis of the magnet = 77° 44′ 42″
Azimuth of true north . . . = 102 48 44

Variation of needle . . . . . = 25° 4′ 2″ west.

The Kater's circle, by means of which these observations were made, has both its vertical and horizontal limbs 6·5 inches in diameter, each furnished with two verniers, reading 10″. The azimuth of the true north was deduced from transits of the sun, taken near the meridian, in the following manner:— The vertical circle being placed approximately in the meridian, the sun's transit over the five diaphragm wires was observed; and the error and rate of the chronometer used were ascertained by comparison with the Edinburgh time-ball within

---

* This paper was read on 19th June 1854.

an hour after observing the sun. The Greenwich mean time of the sun's transit across the theodolite wires was thus obtained,—the correction for the *rate* of the chronometer in the short interval between the observations of the sun and the time-ball never exceeding $0^s{\cdot}1$. The sun's hour-angle, at the instant of his transit across the middle wire of the theodolite, was then easily found from the known longitude of the station; and the deviation of the plane of the instrument from the meridian was calculated with sufficient accuracy by the formula

Sun's hour angle × cos. sun's declination × cosec. sun's zen. dist.

In this manner, on various days, I obtained seven observations for the azimuth of the true north, the greatest difference between any single observation and the mean of the whole being 30″.

I have ventured to give this brief description of the process by which the variation of the needle given above was ascertained, not on account of any novelty it possesses, but merely to enable the reader to judge what degree of reliance is to be placed in the result.

Notes on some Substances which exhibit the phenomena of Fluorescence. By Dr J. H. Gladstone, in a Letter to Dr Anderson.

To Professor Anderson, M.D., F.R.S.E.

My dear Sir,—When I read my short communication "On the Fluorescence exhibited by certain Iron and Platinum Salts," at the Liverpool meeting of the British Association, you kindly offered to publish the results of my observations in the Philosophical Journal. I felt then that my experiments were very incomplete; but since that time I have repeated most of them, examining the blue appearance more critically; and I now transmit to you a fuller description of the phenomena. Will you allow me also to add an account of a few other substances that exhibit fluorescence, but which have come under my notice more recently.

It was while investigating the laws of chemical affinity, that I had occasion to make many mixtures of iron salts, and I observed that several of these exhibited, at the edges of the solution, a peculiar blue dispersion of light, similar to that which occurs in bisulphate of quinine and some other substances, and which, since the admirable research of Professor Stokes, has received the appellation " fluorescence."

Ferrocyanide of Iron in Oxalic Acid.—If ferrocyanide of potassium be added to a salt of the sesquioxide of iron, we all know that a blue precipitate will form; but if this addition be made in the presence of oxalic acid, it is not a precipitate, but a blue solution that results. On the surface, and about the edges of this, appears a blue, which is easily distinguishable from the colour of the solution. It can be shown to best advantage when the citrate of iron is used in its production, since a portion of that salt always remains undecomposed, and, by its green colour, renders the superficial blue more evident, especially if it be in considerable excess. If there be not sufficient oxalic acid, an extremely fine precipitate diffuses itself throughout the liquid, and the blue refraction is more apparent; if the oxalic acid be in very large excess, the solution is perfectly clear and transparent to transmitted light, and it is difficult to observe the blue refraction by reflected light, unless a ray from the sun passing through a slit in the shutter be allowed to fall upon it. A solution of prussian blue in aqueous oxalic acid exhibits the same phenomenon.

It was important, of course, to ascertain whether this blue appearance was due to actual *fluorescence,* or was merely a case of *opalescence* produced by minutely divided solid matter. Upon the slightest inspection of a solution in not very strong oxalic acid, it was evident that there was a large amount of opalescence; therefore, a perfectly clear solution was prepared by the addition of a very large quantity of the vegetable acid, and the following experiments were made.

1*st,* A strong solution of bisulphate of quinine was prepared, and placed in a glass vessel behind a slit in the window shutter, in such a manner as to cause the sun's ray to pass through it. It was found to cut off the fluorescent rays so completely, that a solution of bisulphate of quinine in a test-tube, held in the ray behind it, appeared perfectly free from colour. Another test-tube, containing the solution of ferrocyanide of iron in oxalic acid, was placed in the same position; the internal dispersion was much diminished, though not altogether destroyed, and it was of a greener hue than when the sulphate of quinine was not interposed. This effect did not take place on the interposition of a similar thickness of distilled water. From this it was concluded that there was true, as well as false, internal dispersion.

2d, Thinking that if the solution under examination were itself fluorescent, it ought to cut off the rays capable of producing fluorescence, just as the quinine salt does, I filled two glass vessels of similar form and size, the one with a solution of ammoniacal sulphate of copper, the other with a solution of prussian blue in oxalic acid. They were almost identical in colour and appearance when viewed by transmitted light; when examined by reflected light, the iron solution appeared dull and slightly green, and the result proved that the light which passed through them was really very different in its properties. When a tube containing bisulphate of quinine was placed in a ray of light which had passed through the copper solution, its course was marked by a beautiful fluorescent blue; but when placed in a ray that had traversed the blue iron solution, it remained colourless. When a solution of the prussian blue was itself substituted for the quinine salt in the tube, a like result was obtained; the ray that had passed through the ammoniacal copper solution produced a fine fluorescence, while that through the iron solution exhibited little or no internal dispersion; and so it was with other fluorescent substances. This difference of action of the two blue solutions equally took place when the ferrocyanide was diluted so as to be far lighter in colour than the copper solution.

On casting the prismatic spectrum into a vessel containing prussian blue dissolved in oxalic acid, it was found that the solution transmitted green light in those parts of the spectrum which are ordinarily yellow, and blue in those portions which are usually green or blue; while nothing was transmitted in the violet portion of the spectrum, nor did any luminous appearance present itself (as in the case of bisulphate of quinine) beyond. The peculiar phenomenon of the blue coloration of the liquid itself to a certain depth was exhibited about the *least* refrangible portion of the ordinary blue ray. I should have defined these positions by a reference to Fraunhofer's fixed lines; but the atmosphere was too hazy at this season of the year to admit of my obtaining a sufficiently good and bright spectrum by means that exhibited the lines plainly enough in the summer.

Meconate of Iron.—The red solution that results when a salt of the sesquioxide of iron is added to meconic acid, exhibits on its surface a faint fluorescence. It showed itself when the red was produced by double decomposition from the ferric, nitrate, sulphate, or chloride; but I failed to detect it in the red solution that ensues when sesquioxide of iron itself is dissolved in meconic acid. I believe this arises from the meconate thus produced being a more acid compound; for there are several meconates of sesquioxide of iron, and I found that the iron salt should be in excess to cause the blue, while the addition of a large excess of meconic acid to a fluorescent red solution will cause the appearance to cease.

The blue in this case also seemed to be partially due to opalescence; but it was diminished in a marked manner when a solution of bisulphate of quinine was interposed in the incident ray. When placed behind a screen of ammoniacal sulphate of copper, it displayed an intense blue; but when held behind one of ferrocyanide of iron dissolved in oxalic acid, scarcely any blue was perceptible.

This meconate was found to transmit none of the rays of the prismatic spectrum excepting the red and orange, and the fluorescence in this case appeared to manifest itself about the region where the green passes into the blue.

Gallate of Iron.—A very faint fluorescence is exhibited by the greenish-black solution that ensues when gallic acid is added to a salt of sesquioxide of iron perfectly free from protoxide. The blue fluorescence appears to be unaffected by an excess of either the metallic salt or the acid.

Platinum Salt.—If dilute solutions of bichloride of platinum and of iodide of potassium be mixed together in the proportion of two equivalents of the latter to one of the former, a brownish-red solution results, which exhibits a blue appearance at the edges. If to such a solution successive portions of either the platinum salt, or the iodide of the alkali, be added, the fluorescence gradually diminishes, and eventually ceases to be discernible. What the fluorescent compound here is I have not been able to ascertain with any degree of certainty. Pure biniodide of platinum is insoluble in water, and its solution in iodide of potassium is of a most intense red, without any superficial blue. Probably chlorine in some condition forms a constituent of the compound; indeed the fluorescence was found to be rather increased than otherwise by the addition of chloride of potassium, though the red colour was considerably reduced. That this internal dispersion was not due to mere opalescence, was proved by its not being exhibited when the incident light had passed through quinine salt.

Besides these experiments on iron and platinum salts, I have made several with the specimen of comenamic acid which you were kind enough to give me. I subjoin the results.

Alkaline Comenamates.—I found that an aqueous solution of the acid itself is absolutely devoid of dispersive power upon a ray of light, but that when combined with an alkaline base it is very fluorescent. If excess of potash be added, a blue results which is almost equal to that of bisulphate of quinine; and as the solution is itself perfectly transparent and colourless, it may serve as a good additional means of analyzing light. The blue does not appear when the comenamate of potash is placed in a ray that has traversed a solution of quinine salt, or of prussian blue in oxalic acid; but it is rendered very visible in a ray which has passed through the ammoniacal copper salt. On throwing a prismatic spectrum upon a solution of this salt, the blue dispersion was found to take place in the violet portion of the beam.

The comenamate of soda resembles the potash salt.

The ammonia salt, the epipolic dispersion of which Mr How has mentioned in his paper (Trans. Roy. Soc. Ed. xx., Pt. 2), was also examined. I found it required great dilution to bring out the effect properly; and it did not seem to me to give a blue equal in intensity to that of the compounds with the fixed bases.

Baryta, or lime-water, added in excess to a solution of comenamic acid, likewise gives a fluorescent solution.

Mr How remarks that comenamic acid usually combines with bases in two proportions; and I have strong reason for thinking that it is the more basic compounds that give the appearances above noted.

Comenamate of Iron.—Sesquioxide of iron enters into two combinations with comenamic acid. The more acid one is of a wine-red colour, and exhibits no dispersion; the basic one is of a bluish purple, and when sufficiently diluted becomes fluorescent. Thus, if nitrate of iron in excess be added to comenamic acid, and water be poured on to the deep purple liquid, it will change through claret and pink, and then there will appear some faint blue rays, which are not exhibited when a solution of bisulphate of quinine is interposed. On throwing a prismatic spectrum on to this compound, I did not detect the illumination of any extra-spectral rays.

Comenamate of Quinine.—Considering that so many salts both of quinine and of comenamic acid caused the phenomenon of fluorescence, it seemed not improbable that a compound of the two might give it with increased effect. However, experiment proved it otherwise. To a solution of comenamic acid quinine was added, until a portion of the alkali remained undissolved, even after standing over night. The solution thus obtained showed no dispersive power; on dilution a very faint blue appeared, when viewed in the most favourable positions; but it is quite possible that this may even have arisen from some minute impurity. The addition of sulphate of potash did not revive the blue (at least not to any extent), but the slightest addition of either sulphuric acid, or potash, alone, reproduced it.

When it is remembered that only acid salts of quinine, and only basic salts of comenamic acid (as I apprehend), display the blue, the absence of such an appearance in this compound is less improbable *a priori* than would at first be imagined.

I have added comenamic acid to solutions of several salts of the earths and metallic oxides—including uranium—without being able to observe any other instances of fluorescence; not even in the case of the lead salt, produced from the basic acetate of lead. This, however, cannot be considered as the way most suited for producing fluorescent comenamates.

Sulphate of Uranium.—Among the many fluorescent compounds of uranium, Professor Stokes does not mention the sulphate. I prepared the salt, and found that the crystals gave a fine greenish dispersion in the more refrangible portion of the spectrum, to about the same extent as the nitrate does. A strong aqueous solution was likewise fluorescent, though only to a slight degree.

Phosphate of Phenyl.—In the Quarterly Journal of the Chemical Society, which appeared last month, there is a paper by Mr Scrugham on "Some New Compounds of Phenyl," in which he describes, among other bodies, a tribasic phosphate. It is an oily liquid at the ordinary temperature, and is said to be fluorescent: "By ordinary daylight, the epipolic rays, which have a fine violet tint, are visible at some distance below the surface; the flame of sulphur does not produce this effect more strongly than the light of the sun." Through the kindness of Professor Williamson, I have had an opportunity of examining a fine specimen of this substance. It was clear, but of a somewhat yellow tint; and the dispersed colour was, as stated, not blue, but a very beautiful violet. It was so strong as to be perfectly visible by gas-light: it did not exhibit itself behind a screen of sulphate of quinine, or ferrocyanide of iron solution; and when examined by a ray which had passed through ammoniacal sulphate of copper, it presented an appearance as of a self-luminous, pale violet cloud, entering some distance, perhaps an inch, into the liquid.

Ottar of Roses.—It is well known that many oils, produced by the dry distillation of organic bodies, exhibit a great disposition to internal dispersion. It is in most cases difficult to isolate the particular substance to which the property may be owing. Mr Arthur Church has, however, directed my attention to one hydrocarbon that displays a remarkable fluorescence—the ottar of roses. The blue appearance in this case is similar to that of sulphate of quinine, and is produced or impeded by the same circumstances. Mr Church states that he has by this means determined the presence or absence of this essential oil in various mixtures, where it certainly could not have been detected by ordinary means of analysis.

Thinking that the green exhibited in certain directions by the purple murexide might be due to this cause, we examined it by means of a screen of yellow uranium glass, or of sulphate of quinine solution. Neither of these, however, prevented in any measure the exhibition of the green colour. A solution of murexide in water, also, displays no sign of fluorescence.

I regret that I have been prevented by other scientific engagements from working out the above miscellaneous observations so fully as I could have desired; yet I send you the present notice of them, hoping it may interest some of your readers, and may lead to a further examination of the substances by parties more accustomed than myself to optical experiments. I remain, my dear Sir, yours, &c.

JOHN H. GLADSTONE.

LONDON, *Nov.* 13, 1854.

On Mechanical Antecedents of Motion, Heat, and Light. By WILLIAM THOMSON, Esq., Professor of Natural Philosophy, University of Glasgow. Communicated to the British Association, Section A, Monday, Sep. 28, 1854. [Author's Abstract.]

This communication was opened with some general explanations regarding mechanical energy, and the terms which have been introduced to designate the various forms under which it is manifested. Any piece of matter, or any group of bodies, however connected, which either is in motion, or can get into motion without external assistance, has what is called mechanical energy. The energy of motion may be called either "dynamical energy," or "actual energy." The energy of a material system at rest, in virtue of which it can get into motion, is called "potential energy." The author showed the use of these terms, and explained the ideas of a *store of energy*, and conversions and transformations of energy, by various illustrations. A stone at a height, or an elevated reservoir of water, has potential energy. If the stone be let fall, its potential energy is converted into actual energy during its descent, exists entirely as the actual energy of its own motion at the instant before it strikes, and is transformed into heat at the moment of coming to rest on the ground. If the water flow down by a gradual natural channel, its potential energy is gradually converted into heat by fluid friction, according to an admirable discovery made by Mr Joule, of Manchester, about twelve years ago, which has led to the greatest reform that physical science has experienced since the days of Newton. From that discovery it may be concluded with certainty, that heat is not matter, but some kind of motion among the particles of matter; a conclusion established, it is true, by Sir Humphrey Davy and Count Rumford, at the end of last century, but ignored by even the highest scientific men during a period of more than forty years. Mr Joule, by a series of well-planned and executed experiments, ascertained that a pound of water would have its temperature increased by 1° (Fahrenheit), if it kept all the heat that would be generated by its descent in the way described above through 772 feet; that is, the "actual" or "dynamical" energy of as much heat as raises by one degree the temperature of a pound of water, is an exact equivalent for the potential energy of a pound of matter 772 feet above the ground. Mr Joule also fully established the relations of equivalence among the energies of chemical affinity, of heat of combination or of combustion, of electrical currents in the galvanic battery, of electrical currents in magneto-electric machines, of engines worked by galvanism, and of all the varied and interchangeable manifestations of calorific action and mechanical force which accompany them. These researches, with the theory of animal heat and motion in relation to the heat of combustion of the food, and the theory of the phenomena presented by shooting stars, due to the same penetrating investigator, have afforded to the author of the present communication the chief groundwork for his speculations.

The heat emitted by animals, and the mechanical effects which they produce, are transformations of the energy of chemical affinity with which the food consumed by them combines with the oxygen they inhale. The heat, sound, and mechanical effects, produced by the explosion of gunpowder are, all together, equivalent to the energy of chemical affinity between the different substances of which the unburned powder is composed. The potential energy of war is contained in the stores of gunpowder and food which are brought to the field. The gunpowder carried by artillery and infantry contains all the potential energy ordinarily brought into action by those two arms of the service. The men's food, and the forage for the horses, contain the stores of potential energy drawn upon in a charge of cavalry. Artillerymen, foot-soldiers (unless employed to make a bayonet charge), sailors, steamers with their engines, guns, swords, are only means and appliances by which the potential energy contained in the stores of gunpowder and food is directed to strike the blows by which the desired effects are produced.

The heat and mechanical actions of animals are transformations of the potential energy of their food, mechanically equivalent to the heat that would be got by burning it. The food of animals is either vegetable, or animal fed on vegetable, or ultimately vegetable after several removes. Now,—except mushrooms and other funguses, which can grow in the dark, are nourished by organic food like animals, and absorb oxygen and exhale carbonic acid, like animals,—all known vegetables get the greater part of their substance, certainly all their combustible matter, from the decomposition of carbonic acid and water absorbed by them from the air and soil. The separation of carbon and of hydrogen from oxygen in these decompositions is an energetic effect, equivalent to the heat of recombination of those elements by combustion, or otherwise. The beautiful discovery of Priestley, and the subsequent researches of Sennebier, De Saussure, Sir Humphrey Davy, and others, have made it quite certain that those decompositions of water and carbonic acid only take place naturally in the daytime, and that light falling on the green leaves, either from the Sun or from an artificial source, is an essential condition, without which they are never effected. There cannot be a doubt but that it is the dynamical energy of the luminiferous vibrations which is here efficient in forcing the particles of carbon and hydrogen away from those of oxygen, towards which they are attracted with such powerful affinities; and that luminiferous motions are reduced to rest to an extent exactly equivalent to the potential energy thus called into being. Whether or not the coolness of green fields and fresh foliage is, to any sensible extent, due to this cause, it is quite certain that sun-heat is put out of existence as heat, by the growth of plants in any locality; and that just as much heat, neither more nor less, is emitted from fires in which the whole growth of any period of time is burned. Coal, composed as it is of the relics of ancient vegetation, derived its potential energy from the light of distant ages. Wood fires give us heat and light which has been got from the Sun a few years ago. Our coal fires and gas lamps bring out, for our present comfort, heat and light of a primeval Sun, which have lain dormant as potential energy beneath seas and mountains for countless ages.

We must look, then, to the Sun as the source from which the mechanical energy of all the motions and heat of living creatures, and all the motion, heat, and light derived from fires and artificial flames, is supplied. The natural motions of air and water derive their energy partly no doubt from the sun's heat, but partly also from the earth's rotatory motion, and the relative motions and mutual forces between the earth, moon, and sun. If we except the heat derivable from the combustion of native sulphur and of meteoric iron, every kind of motion (heat and light included) that takes place naturally, or that can be called into existence through man's directing powers on this earth, derives its mechanical energy either from the Sun's heat or from motions and forces among bodies of the solar system.

In a speculation recently communicated to the Royal Society of Edinburgh, the author had shown that the Sun's heat is probably due to friction in his atmosphere between his surface and a vortex of vapours; fed externally by the evaporation of small planets in a surrounding region of very high temperature—which they reach by gradual spiral paths; and falling inwards in torrents of meteoric rain from the luminous atmosphere of intense resistance, to his surface.

A continuation of the inquiry raises the question, from what source do the planets, large and small, derive the mechanical energy of their motions? This is a question to the answering of which mechanical reasoning may legitimately be applied. For we know that from age to age the potential energy of the mutual gravitation of those bodies is gradually expended, half in augmenting their motions, and half in generating heat; and we may trace this kind of action either backwards or forwards—backwards for a million of million of years with as little presumption as forwards for a single day. If we trace them forwards we find that the end of this world as a habitation for man, or for any living creature or plant at present existing in it, is *mechanically inevitable;* and if we trace them backwards according to the laws of matter and motion—certainly fulfilled in all the actions of nature which we have been allowed to observe—we find that a time must have been when the earth, with no Sun to illuminate it, the other bodies known to us as planets, and the countless smaller planetary masses at present seen as the zodiacal light, must have been indefinitely remote from one another, and from all other solids in space. All such conclusions are subject to limitation, as we do not know at what moment a creation of matter or energy may have given a beginning, beyond which mechanical speculations cannot lead us. If in purely mechanical science we are ever liable to forget this limitation, we ought to be reminded of it by considering that purely mechanical reasoning shows a time when the earth must have been tenantless, and teaches us that our own bodies, as well as all living plants and animals, and all fossil organic remains, are organized forms of matter to which science can point no antecedent except the will of a Creator, a truth amply illustrated by the evidence of geological history. But if duly impressed with this limitation to the certainty of all speculations regarding the future, and prehistorical periods of the past; we may legitimately push them into endless futurity, and we can be stopped by no barrier of past time without ascertaining at some finite epoch a state of matter not derivable from any antecedent by natural laws. Although we can conceive of such a state of all matter, or of the matter within any limited space, and have cases of it in the arbitrary distributions of temperature prescribed as "initial" in the theory of the conduction of heat (see* Cambridge Mathematical Journal, Vol. IV., p. 67, 1843) yet we have no indications whatever of natural instances of it; and in the present state of science we may look for mechanical antecedents to every natural state of matter which we either know or can conceive at any past epoch, however remote.

It is by tracing backwards the motions which are at present observed, according to the known laws of motion and heat, with no limit as to time, that the author arrives at the conclusion that the bodies now constituting our solar system have been at infinitely greater distances from one another in space than they are now. He remarked, that the nebular theory, as ordinarily stated, assuming as it does a previously gaseous state of matter, is not only untrue, but the reverse of the truth, according to the views now brought forward; since these show evaporation—as a necessary consequence of heat generated by collisions and friction, and the general past and present tendency of matter is seen to be the conglomeration of solids and liquids, accompanied by a gradual increase of the density of gaseous fluid evaporated through space.

Professor Helmholz, in a most interesting popular lecture on transformations of natural forces, delivered on the 7th of February last at Königsberg, has estimated that, if the particles at present constituting the Sun's mass have been drawn together by mutual gravitation from a state of infinite diffusion, as assumed in the nebular theory, (not however a gaseous state, as ordinarily supposed, but a state in which the particles exercise no mutual action except that of gravitation,) the whole heat generated must have amounted to about 28,000,000 thermal units centigrade per pound of the sun's mass. This estimate would not, as the author of the present paper shows, require any change, whether we assume as the immediately antecedent condition of the Sun's matter a state of infinite diffusion or a state of aggregation in solid masses of any di-

---

* "Note on some points in the Theory of Heat," a short article in which it was shown how to test the *age* of a distribution of heat, by applying a certain criterion of convergence to its expression in the infinite series, characteristic of the external circumstances of the body in which it is given.

mensions small compared with his present dimensions, and separated from one another at comparatively great distances, provided always there has been no relative motion among them except what is generated by mutual gravitation. If, then, the whole mass of the Sun has grown by the process which, according to the author's theory (*certain* as regards a part, whether or not it may be sufficient to account for the whole, of the radiation) of solar heat, we know to be augmenting it at present, there must have been generated in the whole process of conglomeration the quantity of heat stated above, a quantity which amounts to about 20,000,000 times as much as is at present radiated off in one year. The author gave reasons for believing that this heat has probably been nearly all radiated off immediately on being generated; and that enough of it has not been retained in the conglomerated mass to be the store from which the heat at present radiated is drawn.

That the present solar radiation is supplied chiefly from a store of heat contained in the mass, whether created there or generated mechanically by the impacts of meteors which have fallen in during remote periods of past time, appears very improbable.

On the contrary, there must in all probability be some agency continually supplying heat to compensate the loss constantly experienced by radiation from the Sun; and that agency,* as the author has shown elsewhere, can be no other than the mechanical action of masses coming from a state of very rapid motion round the Sun, to rest on his surface.

---

* It is quite certain that it cannot, as the nebular theory has led some to suppose it may, be the energy of gravitation effecting any continued condensation of the Sun's present mass, since without increased pressure, it is only by cooling that any condensation can be taking place; and the heat emitted in consequence of condensation by cooling, would depend merely on the specific heat of the whole mass in its actual circumstances of temperature and pressure, and might, (for all we know of the properties of matter at such high temperatures and pressures) be greater than equal to, or less than, the thermal equivalent of the work done by gravity on the contracting body. Thus the heat emitted by a mass of air, contracting under any constant pressure, is greater than the amount mechanically equivalent to the work done by the pressure. The heat emitted by a mass of water, or of mercury, cooling from 100° to 50° Cent. under any constant pressure exceeding about 90,000 atmospheres, is less than the amount of heat mechanically equivalent to the work done by the pressure on the contracting mass.

The author showed how a system of solid bodies, large and small, initially at rest, and at great distances from one another, may, by their mutual gravitations, and by the resistance their motions must experience in the gaseous atmosphere evaporated from them by the heat of their collisions, after a vast period of time come into a state of motion, heat, and light, analogous to the present conditions of our solar system and of the visible stars.

The origin of rotatory motion is explained by showing that different systems starting from rest will influence one another so as to acquire contrary rotatory motions without any aggregate of rotatory momentum being acquired by the whole. Any system or group beginning to concentrate round one principal mass, after having thus acquired a momentum of rotatory motion, will acquire from it, in a certain stage of advancement, just such approximately circular motions as those of the planets, the particles of the zodiacal light, and the satellites of our solar system, and such rotatory motions as the central and other masses are known to have, all chiefly in one direction.

In considering the question whether all the heat and motion at present existing in matter have their origin in that action by which their amount is at present being increased, it is shown that unless their entire actual energy exceeds a certain definite limit, namely, the value of the whole potential energy of gravitation that would be spent in drawing all the particles of matter from a state of infinite diffusion into their present positions, it is quite possible they may be so produced—or, *that the potential energy of gravitation may be in reality the ultimate created antecedent of all the motion, heat, and light at present existing in the universe.*

Further Observations on Glacial Phenomena in Scotland and the North of England. By R. Chambers, F.R.S.E.

In a former paper, read to the Royal Society of Edinburgh, and published in Jameson's *Philosophical Journal* for April 1853, I endeavour to establish a distinction between an early general operation of ice over the surface of Scotland, leaving the compact boulder clay as its monument, and a more recent presence of valley glaciers in our chief mountain systems, the detritus of which was of a lighter, looser, and coarser texture, indeed identical in character with the moraines of existing Alpine glaciers. The latter glacial operation I considered as for certain taking place without the presence of the sea, and in circumstances which admitted of a constant drainage from the body of the glaciers. The former I deemed as showing, in its effects, that either the sea was present, or that the ice, covering a large surface of country, and consequently having no drainage comparable to that of a valley glacier, retained its own water within or about itself, so that the circumstances were not greatly different from what we might expect in large floods of sea-borne ice. It must remain an obscure problem how ice can move over large surfaces of country; but that its so moving is a fact of nature, has, since the preparation of my paper, been remarkably confirmed by the observations of Dr H. Rink of Copenhagen in the west of North Greenland, where he has found what may be called a continental glacier of vast thickness, continually advancing from the interior of the country to the coast, and there breaking off in icebergs.

I have been able this summer to make a few observations tending further to illustrate the distinction which I endeavoured to establish between the compact boulder clay, as a memorial of early, general, and watery glaciation, and the coarse brown drift, as a monument of subaërial valley glaciation, exactly resembling that seen in the Alps.

True moraines had been scarcely detected in Scotland before Mr Maclaren described that of Glenmessan, in Argyleshire, to the geological section of the British Association in 1850. The only examples adverted to before that period were those pointed out by Sir Charles Lyell, as forming the dams of Lochs Brandy and Whorral, two mountain tarns, placed high on the eastern skirts of the Grampians, and one or two specimens observed by Professor James Forbes on the skirts of the Cuhullin hills, in Skye. In the paper just adverted to, I described some examples of moraines which I had discovered in the wilds of Skye, of Sutherlandshire, and Ross-shire. Since then, some others have come under my attention, and I am able to put this kind of phenomena under a certain degree of classification.

In at least two of the valleys descending from the skirts of

Ben Macdui, in Aberdeenshire, there are conspicuous moraines of the terminal order, which have been left by their respective glaciers at various stages of their shrinkings. In Glen Dearg, there are fully four unmistakeable masses of detritus of this kind, a mile or two apart from each other, and which would each block up the valley entirely, and form a lake of its waters, if these had not been able, in the course of time, to make a passage through them. I measured the height of one of these masses, and found it 130 feet. The bottom of this valley must be about 1700 feet above the level of the sea.

Valley glaciers have, however, descended much below this level. At a much lower point in the valley of the Dee, namely, in the side vale of the Muick, and on the property of the Prince of Wales, there is a scarcely less remarkable series of moraines. In this case the glacier would be partly fed from the skirts of Lochnagar.

In the valley of the Tay, masses of moraine matter first attract attention a little below Aberfeldy, not much more than 300 feet above the sea-level. In the tributary valley descending from the skirts of Schihallion, near Garth castle, are some of the higher, and consequently more recent, terminal moraines of what must have been a feeder of the same glacier.

Few as are the other examples of valley moraines in Scotland, which have come under my observation since the publication of the former paper, it would be tedious to enumerate them. They are such, however, as to show that wherever there are mountains in Scotland approaching or exceeding 3000 feet in height, there have glaciers been, tearing away detritus, and leaving it in large accumulations.

Such is one class of Scottish moraines. There is another class connected with bosoms or recesses of the more elevated class of mountains, being usually placed in front of these as a fender is placed before a fire. In such recesses we are to presume that masses of snow have gathered, till they became so great that a movement outward took place. With this movement, perhaps not a mile in extent, often only a few hundred yards, came detritus, which of course rested at the outskirts of the mass. Of this character was a moraine which I formerly described as existing in Benmore Coigach near

Ullapool. Such too are the moraines which confine Lochs Whorral and Brandy. On lately visiting for the first time the well-known Loch Skene in Dumfriesshire, which resembles the above two lakes in situation, I found it to be formed by a moraine of this order. The hills are here about 2600 feet high, being the loftiest in Scotland south of the Forth. In a south looking recess, backed by a lofty wall of bare rock, and on a platform which cannot be less than 1200 feet above the sea lies this celebrated lake, hemmed in towards the south by a bewildering number of hillocks and ridges of gray coarse drift, the manifest spoils of the ice which once filled the recess. In front of a similar *sinus* to the westward, we have the same lines and humps of detritus; but the water has there made a passage for itself and escaped. This passage is as clearly defined as a gate in a wall or a drain in a field.

In the Island of Arran, near the mouth of the valley of Loch Ranza, and not more than fifty feet above the sea, there is a line of detritus of, perhaps, a furlong in length, and cut down by an opening in the centre. It faces to a north-looking recess in the hills, and is doubtless the moraine of the glacier (if it can be so called) which once filled that recess. The intervening space, which is of no great extent, is now occupied by a morass.

To this class of objects, which are also very common in Scotland, the name of *Recess Moraines* might perhaps be considered appropriate.

For the satisfaction of English geologists, few of whom may have opportunities of going over my ground in Scotland, it is well that I can point to an object in England bearing all the characteristics of a moraine. It is not of the terminal kind, like those in the Ben Macdui valley of Glen Dearg, but a very perfect example of a lateral moraine, or moraine left on the side of a glacier. I must transport my readers to the centre of the Lake District, where occurs the well-known col or pass named Dunmailraise, 720 feet above the level of the sea. Valleys descending from the adjacent mountain range, including the Langdale Pikes, and Borrowdale Fells,—go, one to the east by Grassmere, and another to the west by Thirlmere, leaving the cross valley or valley of passage, of which Dunmailraise is the summit, extending about four miles between. Now, where the Thirlmere valley enters this valley of passage, near the Wythburn Inn, we see a remarkable-looking double ridge descending the hillside. It is prominent above the general outline of ground, to the height of about thirty feet, and its surface is bristled with blocks. This double ridge precisely answers in form and relative situation to the character of such a moraine as that of Les Tines, connected with the Glacier des Bois of Chamouni. It is the train of detritus which a glacier three or four hundred feet deep, coming down the Thirlmere valley, would throw over and leave, at two stages, on the face of the valley of passage in which we find it. The constituent rocks are the same as those of the valley. Further down the vale of Thirlmere, there are many other heaps of the like detritus (along with rounded, grooved, and scratched rocks), but none that take so significant a form as this. It is also to be observed that glacialised rock faces abound in the valley above the point where it joins the valley of passage.

Some late observations tend to confirm my former proposition, that there are two sets of glacial phenomena, widely different in extent, and separated in time.

The well-known mountain of Schihallion in Perthshire, rises from a plateau about 1100 feet above the sea, to the height of 3600 and upwards. It is composed of quartz rock, and to the comparative hardness is due the preeminence, which (with one exception), it has over all the neighbouring mountains. This great mountain is abrupt to the westward, and *tails* away to the east, precisely like the many hills in the valley of the Forth, which are regarded as taking their form from glacial action. The top of the ridge being thickly strewn with loose slabs, shows such a tendency to peeling under a denuding agent, that it seems a very unlikely place for the discovery of glacial smoothings and scratchings. Nevertheless, I found surfaces at several places, bearing that peculiar streaking which I had remarked some years ago as a glacial phenomenon peculiar to quartz rock on the mountain of Queenaig in Assynt. About half way up from the plateau, and certainly not less than 2200 feet above the sea,—a point where we are above the summit of Ferragon, the most conspicuous mountain to the eastward, and likewise many of the sky-lines, both to the north and south,—there was one fine group of examples. There is another similarly striated or streaked surface only a few hundred feet below the summit of the hill. The direction of the striation in both instances is W. 30 N., being the general direction of the ridge of which the mountain consists. About 800 feet below the summit, I found a block of granite, and in several other places there were blocks of other rocks likewise different from those of which the hill consists. From all I have seen, I can entertain no doubt that Schihallion owes its form to a glacial agent which has engulfed its whole mass.

It has been related that humps of brown moraine detritus are found in the vale of the Tay, and in the tributary valley which ascends to the skirts of Schihallion. Such is a general condition of valleys in relation to similar mountain groups in Scotland. On coming, however, to a *col* or summit-level on the eastern skirts of the mountain (a place called White Bridge, fully 1000 feet above the sea), we find a total change in the character of the detritus. Here we have the unmistakeable blue boulder clay, a deep bed, enclosing blocks of various sizes, but none very large, all worn smooth and many of them striated. This spot is a pass between valleys—consequently lies out of the way of any common valley glaciers, such as those which I believe to have deposited the brown moraine matter just adverted to. It has been spared by these glaciers, and allowed to retain its share of that other covering elsewhere found so universal in Scotland. Such, at least, is the only reading which I can give to the facts.

In my former paper, I adverted to a peculiar mass of detritus resting in the valley of passage at Dunmailraise. That place, I said, had been out of the scope of the glaciers which I showed must have once filled the valleys of the lake district, and it had been allowed to retain a detritus probably resulting from some earlier operations. By a second and more careful observation, I have now satisfied myself that this mass of detritus is greatly different from that composing the remains of moraines at Grasmere, a point within the scope of the valley glacier, descending to the east from the bosoms of the Langdale Pikes. While both are fundamentally red clays, and both of a compact character, the latter is less compact than the former, contains a greater number of blocks, and these reaching a much greater size, and many of them smoothed and striated—a peculiarity I could not detect in any of those which rest in the deep mass of detritus nearer the summit of the pass. It is possible that that mass, then, may be composed of washings from the adjacent hill sides, effected at a time when this valley was a sound; but I incline rather to class it with such phenomena as Professor Ramsay has found in Wales, and myself in Scotland, and which I regard as relics of an old clay, due to a general glacial action over the surface of the country, and which has been removed and replaced by a looser material in all parts possessed of the requisites for ordinary local glaciation, namely a certain elevation, and the existence of high valleys or bosoms of the hills sufficiently wide to serve as the *berceaux* of glaciers.

On the Great Terrace of Erosion in Scotland, and its Relative Date and Connection with Glacial Phenomena. By R. Chambers, F.R.S.E.

A terrace of erosion is very conspicuous along the coasts of Scotland, at between twenty and thirty feet above the present level of the sea. It is well marked along the Firth of Clyde, including the islands of Bute and Arran, and generally on the coasts of Argyleshire. On the east coasts of Scotland, which are well known to be of so much less bold a character than those of the west, it is less remarkable; yet it has been traced on the Firths of Dornoch and Cromarty, while it may be said to have an equivalent in the Firth of Forth, in the well-known bench of land, of about the same elevation, which stretches along both sides of that estuary. Everywhere, as is well known, shells of the present epoch are found on this terrace.

On the western coasts of Scotland, the breach which it makes in the outline of the land is very striking. Generally the hills slope in a smooth line to the sea, and very often we find this slope but little broken even where the sea is now in contact with the land. But between the slope above and the only somewhat more broken slope of the existing beach below, this line of erosion forms generally a well-defined rectangular cut; that is to say, a space composed of a vertical cliff rising from a level platform. This cliff is in many places forty feet high; but in some, is not less than a hundred; while the platform is seldom less than a hundred feet broad, thus affording sheltered situations for hundreds of villas to the wealthy inhabitants of Glasgow. Where composed of sandstone, the cliff is apt to be perforated in pretty deep caves, many of which are memorable in the history of the smuggling trade. In some places in Arran, where the cliff is of this rock, huge slabs are left prominent above, hanging over like a pent-house. On other parts of the coast, where mica slate prevails, a harder mass of that formation, or a mass of upthrown trap, will be found starting up in some fantastic form from the platform; and occasionally there is a crowd of such objects.

The very great amount of attrition borne witness to by this terrace, in comparison with that which can be traced on the present coast line, shows that it must have been the meeting-point of sea and land during a much longer space of time than the present beach. We know on tolerably good grounds that the existing relative level of sea and land has been unchanged during the historical era, or for not much less than two thousand years. It follows that the space of time during which this terrace was the sea beach, must be some large multiple of two thousand years, if not of something considerably more. It seems as likely to have been the beach for ten thousand years as the present is for the fifth of that period.

The immediate object of this paper is to exhibit proof that the formation of this terrace is not an event immediately prior to the assumption of the present line of relative level between sea and land, but one of some antiquity in the post-tertiary epoch.

In passing along the north-west coast of Arran, from Loch Ranza southwards, we have the ancient sea-cliff rising like a wall of from fifty to a hundred feet high all the way, sometimes bare as when it was left by the dash of the sea, in other places feathered with fern, and birch, and the mountain ash; in all places striking and picturesque. In his progress, the eye of the observer is suddenly arrested by the appearance of something like the boulder clay resting on the face of the cliff. The mystery begins to clear when he finds that he is close to the opening of a Highland valley called Glen Iorsa, the mouth of which is filled to a considerable height with terraces of detritus, and which stretches back to the lofty mountains forming the centre of the island. On an examination of these detrital masses, he finds the lower part composed of a bed of blue clayey drift, with small half-worn boulders scattered equally through it; over this, a bed of coarse gravel, and above this again, a deep bed of fine sand. The stuff which he had seen on the face of the ancient sea-cliff, is the same with the first of these deposits. It consequently becomes evident that the three sets of conditions which gave rise in succession to the bed of blue clayey drift with boulders, the coarse gravel, and the deep bed of fine sand, are all posterior to that incising action of the sea which formed the terrace and sea-cliff. I presume it will be readily admitted that the two higher beds argue a period of submergence; and as the surface of the whole is not less than 140 feet above the present sea-level, the submergence must have been to that depth at least. If I am right in considering the blue clayey drift as the product of a glacier which once filled Glen Iorsa, then we had previously had a period of low temperature in Arran. Thus, we may speculate on a glacial period, and a period of deep re-immersion, as following in succession upon the period of this terrace of erosion. Nor is this the whole series of events; for the two superficial deposits argue each a separate set of conditions, the coarse gravel marking a time when the embouchure of the river was little way of the valley, and the bed of fine sand a time when it was much farther up, and when the sea at this place was of course considerably deeper. It is scarcely necessary to remark that the time required for this succession of events must have been very great.

The arrangement of events here speculated upon has support in certain observations of a similar kind which have been indicated by other geologists in Scotland. Mr Milne Home found the deep ravine of the Water of Leith, at the Dean near Edinburgh, overlaid by what he considered as a third drift bed connected with erratic boulders, and argued of course that the cutting of that ravine by the river had been followed by a period of deep reimmersion. Mr Charles Maclaren made a similar remark regarding the ravine of the river Allan between Dumblane and Stirling; but for this I have unfortunately mislaid my reference, so that I can only vaguely indicate the fact.

Geological Survey of Great Britain.

The extension of the Geological Survey of Great Britain to Scotland has for some time been anxiously looked forward to by many persons interested in the subject, whether in a scientific or purely practical point of view. We have now the satisfaction of stating that arrangements have been made for commencing the execution of this work, which has long been in contemplation.

In the year 1845, the geological survey of the United Kingdom was remodelled under Sir Henry De la Beche as Director-General, and, since that date, the Irish branch of the survey has been carried on, first by Captain James, as local director, then by Professor Oldham, and, since the appointment of the latter to the geological survey of India, by Mr Jukes. During the same period, the local directorship of the Geological Survey of Great Britain has been entrusted to Professor Ramsay.

Before that time, a large area had been completed by Sir Henry de la Beche and his staff as then constituted. The survey originally commenced in Devon and Cornwall, and it is worthy of record that the greater proportion of the work in these counties was executed almost by the unassisted efforts of that distinguished geologist. Since then, nearly a half of England and Wales has been geologically mapped, principally in the southern, western, and midland counties, and the survey is now rapidly progressing to the east and north.

The extension of the work to Scotland has never been lost sight of, but it was not till the ordnance survey had made considerable progress, that it was practicable to commence the geological survey there. Several counties have lately been topographically surveyed and published on a scale of six inches to a mile. This great work is rapidly advancing, and on its steady prosecution over contiguous areas, the progress of the geological survey will in a great measure depend; for practical geologists well know that it is generally impossible to carry on large geological investigations with effect with reference merely to the conventional outlines of counties. The workman must follow his lines wherever they lead him; and if brought to a stand for want of maps, till the want is supplied, it may be that he cannot properly apprehend and express their import.

The unavoidable delay that has occurred in commencing this part of the survey, is in a measure an advantage to Scotland, for the topographical maps now finished are not only wonderful specimens of accuracy and beauty, but being on a scale of 6 inches to a mile, they will afford the geologist every facility for laying down all needful details, a matter of no small value in a country, the coal fields of which are so important. Valuable as are the results that have been obtained in England, those interested in its coal fields constantly feel the want of a map larger than the 1-inch scale that alone exists for the central and western counties. Indeed in all matters requiring great detail, such, for instance, as the delineation of numerous faults and coal crops, it is manifestly too small, and in this respect, Scotland will have a great advantage, especially if those whose duty it is to decide on the scale of the Ordnance Maps, for future publication, do not fall into the opposite extreme, and decide upon the engraving of maps too large for the use of geologists who, in the course of a day's work, often require to carry with them maps representing an area of perhaps 100 square miles.

Professor Ramsay personally commenced operations in East Lothian towards the close of the year just terminated. It is impossible to prosecute geological investigations in the field with much effect through the inclement months of winter, but it is anticipated, that during the coming summer, the work will be carried on as vigorously as the means at the disposal of the survey will allow. In investigations of this sort, large results are not to be immediately looked for. They are the work of time; but looking to what has been done and is now doing in England, we confidently expect that at no distant period they will be of a kind worthily to satisfy the expectations of the public.

On the Action of Organic Acids on Cotton and Flax Fibres.

By F. CRACE CALVERT, F.C.S., M.R.A. of Turin, Professor of Chemistry, Royal Institution, Manchester.

I am induced to publish the facts contained in this paper, because they are interesting in themselves, and are likely to prove important in certain arts, especially that of calico-printing; for it appears, contrary to the generally received opinion, that the organic acids exert a corrosive action on cotton and flax fibres, which, in some instances, is nearly as marked as that of the weaker mineral acids.

My attention was drawn to this subject by having a cambric handkerchief placed in my hands for examination, the texture of which was injured in all such parts as had been in contact with an isinglass jelly, sold by a confectioner as made from calves' feet. I soon ascertained that the jelly had been clarified with tartaric acid, and not with any mineral acid; therefore I made a series of experiments with jellies prepared by myself, and compared them with others procured from some of the most respectable confectioners of our city, and I found, as a rule, that cambric linen was materially injured when it had been dipped in such a solution dried in the atmosphere, and then heated to 126° C.

As this interesting fact involved a question of great practical value to the calico printer, I deemed it my duty to examine carefully the action of various organic acids on fibres, and the following pages contain the results of my inquiry.

The first question which presented itself was, whether the injury of the fibres arose from the tartaric acid contained in the jellies, or was to be attributed to the mechanical effect of a solid substance interposed between the fibres of the fabric interfering with their ordinary elasticity, and thus rendering them brittle.

To appreciate the influence of tartaric, citric, and oxalic acids, I dipped small pieces of cambric and muslin (previously well washed in distilled water) into a solution containing two per cent. of tartaric or oxalic acids, carefully purified, and completely free from mineral acids. The pieces were then dried in the atmosphere, and exposed for an hour to various temperatures, and the results obtained are shown in Table I.

Table I. illustrates an interesting fact, viz., that while two per cent of tartaric and citric acid have but a slight action on cotton and flax fibres at 80°, 100°, and 126° C., oxalic acid has a decidedly injurious action, the slightest effort being sufficient to tear the fabric. In fact, the fibres were nearly as much injured as if they had been acted on by a weak mineral acid.

In order to ascertain what quantities of citric and tartaric acid were required to weaken materially cotton and flax fibres, I employed solutions of these acids containing four per cent. of each, and pieces of fabrics were dipped in such solutions, dried in the atmosphere, and submitted to the action of heat. The results are contained in Table II.

These results left no doubt that two per cent. oxalic acid acted on the fibres with still more intensity than four per cent. of citric and tartaric acids; and at the temperature of 126° C. all the fabrics presented a scorched appearance, and those with tartaric and citric acids had assumed a much browner tinge.

To enable me to form an opinion whether the coloration of the linen was owing to the action of the acid on the fibres, or to partial decomposition of the acid itself, I took some of the scorched pieces of fabric, and boiled them with distilled water. The coloration not disappearing, I added a little caustic alkali, but without any better results. I therefore conclude that the coloration of the fabric was attributable to the action of citric and tartaric acids, or to some of their derivative compounds.

The next series was made by dipping for a few minutes pieces of fabric in solution of isinglass, glue, gum, and starch, of the best quality, and having a specific gravity of 1·020, at 37° C. These pieces, after being well pressed and dried in the air, were submitted to the temperatures of 80°, 100°, and 126° C., by which they were found to be somewhat weakened, but the action was so very slight, that by exposure to the atmosphere for a few hours, or by washing out the stiffening substance, they were found to have recovered their primitive strength.

As in calico-printing, oxalic, citric, and tartaric acids, are applied to fabrics when mixed with a stiffening substance, a series of experiments was made with solutions of tartaric, citric, and oxalic acids, thickened with gum and starch, and it was found that the presence of the latter substances greatly increased the action of the above acids, when employed in the proportions of from two to four per cent. on cotton and flax fabrics, and added to their scorched appearance.

The results observed are shown in Table III.

TABLE I.

	80° C.		100° C.		126° C.	
	Linen.	Cotton.	Linen.	Cotton.	Linen.	Cotton.
Immersed in water alone, .	Uninjured.	Uninjured.	Uninjured.	Uninjured.	Uninjured.	Slightly injured.
Water containing 2 per cent. Tartaric Acid, . .	Do.	Very slightly injured.	Do.	Very slightly injured.	Do.	Do.
.. ... Citric Acid,	Do.	Do.	Very slightly injured.		Slightly injured.	
... ... Oxalic Acid,	Rather injured.		More injured.		Very much injured.	

TABLE II.

	87° C.		100° C.		126° C.	
	Linen.	Cotton.	Linen.	Cotton.	Linen.	Cotton.
Water containing 4 per cent. Tartaric Acid, . .	Slightly injured.		Very much injured.		Much injured.	
... ... Citric Acid,	Very slightly injured.		Much injured.		Much injured.	Very slightly injured.
... ... Oxalic Acid,	Much injured.		Very much injured.		Quite rotten.	

TABLE III.

	80° C.		100° C.		126° C.	
	Linen.	Cotton.	Linen.	Cotton.	Linen.	Cotton.
GLUE—						
Containing 2 per cent. Tartaric Acid, . .	Slightly injured.	Apparently uninjured.	Much injured.		Very much injured. Still very strong.	Slightly carbonized.
... ... Citric Acid,	Very slightly injured.		Not much injured.			
... ... Oxalic Acid,	Injured.					
GUM—						
Containing 2 per cent. Tartaric Acid, . .	Slightly injured.		More injured than the corresponding experiment with glue.		Very much injured, and of a brown tinge in parts.	
... ... Citric Acid,	Very slightly injured.		Not much injured.		Slightly discoloured in parts.	
... ... Oxalic Acid,	Much injured.		Very much injured.		Tenacity quite destroyed.	
ISINGLASS—						
Containing 2 per cent. Tartaric Acid, . .	Very slightly injured.		Not much injured.		Much injured, but not discoloured.	Very slightly discoloured.
... ... Citric Acid,	Not perceptibly injured.	Very slightly injured.	Slightly injured.		Injured, but not discoloured.	Do.
... ... Oxalic Acid,	...		...		...	

The above experiments were undertaken with the view of throwing some light on what is sometimes observed when fabrics printed with the above acids are passed over heated cylinders or plates; and I deemed it advisable to inquire also into their action when applied to goods which were simply dried in the atmosphere and afterwards steamed, as is often the case in block-printing. For this purpose I prepared two series of experiments similar to those above described, taking care to separate the specimens, by first wrapping each in paper, and then placing them between folds of white calico. These samples, so arranged, were then submitted respectively for half an hour to steam having 3, 12, and 45 lb. pressure; and the results, which are contained in the subjoined table, were very surprising, as the fibres were found to be much more injured than when they had been submitted to dry heat.

	STEAM AT A PRESSURE OF	
	3 lb.	45 lb.
Water alone, . .	Uninjured.	Uninjured.
.. 2 p. ct. Tartaric Acid, .	Slightly injured.	Much more injured.
... 4 p. ct. Do. .	Do.	Do.
... 2 p. ct. Oxalic,	Very much injured.	Rotten.
... 4 p. ct. Do. .	Rotten.	Very rotten.
Gum alone, . .	Uninjured.	Uninjured.
... 2 p. ct. Tartaric Acid, .	Not more injured than water + 2 p. ct. tartaric acid.	Slightly injured.
... 4 p. ct. Do. .	Same as 4 p. ct. tartaric acid and water.	Injured, but still rather strong.
... 2 p. ct. Oxalic,	Rather more injured than water + 2 p. ct. oxalic acid.	Rotten.
... 4 p. ct. Do. .	Very rotten.	Very rotten.
Starch alone, . .	Uninjured.	Uninjured.
.. 2 p. ct. Tartaric Acid, .	Hardly injured at all.	Slightly injured.
... 4 p. ct. Do. .	Very slightly injured.	Rather more injured.
... 2 p. ct. Oxalic,	Not more injured than water + 2 p. ct. oxalic acid.	Rotten.
... 4 p. ct. Do. .	Do. 4 p. ct. Do.	Very rotten.
Water + ⅛ p. ct. Sulphuric Acid, .	Can hardly be handled.	Not tried.
... ½ p. ct. Do. .	Falls to pieces in the hands.	Do.

The facts contained in the preceding paper are interesting, as indicating the extent to which less powerful, though still sufficiently characteristic actions may be overlooked. Hitherto we have gone upon the supposition that the organic acids are entirely without action upon vegetable fibres, and constant use is made of them by the calico-printers in the production of their colours. My observations, however, sufficiently show that they cannot be used for this purpose without injury; and should serve as a warning to avoid their use, and to replace them as far as possible by neutral salts.

In conclusion, I may mention, as somewhat allied to the subject of this paper, that I have succeeded in making use of the difference of the action of weak animal acids on vegetable and animal fibres, as a means of detecting the admixture of cotton and flax with wool. The latter resists an acid which entirely destroys the former. This fact has acquired considerable practical importance from the extent to which mixed fabrics have been introduced of late years.

On a Hermaphrodite and Fissiparous Species of Tubicolar Annelid. By THOMAS A. HUXLEY, F.R.S., Lecturer on General Natural History in the Government School of Mines.

In the course of a series of dredging operations, in which I have lately been engaged, upon the shores of Caermarthen Bay, in the neighbourhood of Tenby, I took, upon one occasion and in one locality (in about six fathoms water, near Proud Giltar), the Annelid which is the subject of the present communication. It is questionable, however, whether the animal is so rare as I might have been led to suppose from this solitary instance of its occurrence within my own knowledge—for I had afterwards the opportunity of seeing masses of its calcareous habitation considerably larger than that which I took myself, in the celebrated collection of the late Mr Lyons of Tenby.

The *Vermidom* (as one might conveniently term the habitations of tubicolar annelids in general) of this annelid is composed of very fine, more or less undulated, white, calcareous tubes, attached by one end to some solid body. Rising from this fixed base, they unite together side by side into irregular bundles, and these bundles anastomose like bundles of nerves in their plexuses—leaving irregular spaces here and there, and thus forming a kind of coarse solid network (fig. 1). Each tube has a circular section, but can hardly be called cylindrical, because it is thickened at intervals, so as to be obscurely annulated.

When placed in a vessel of clear sea-water, the annelids issue from the tubules of their vermidom, and each spreading out its eight branchial filaments and displaying its bright red cephalic extremity—the mass assumes a very beautiful and striking appearance—singularly resembling a tubuliparous polyzoarium (fig. 2).

If, however, a portion of the calcareous mass be broken down, and its delicate fabricators carefully extracted (fig. 3), their annelidan nature becomes immediately obvious; and in determining the exact place of this form among the tubicola, the expanded membrane which fringes the sides of the body, the peculiar branchial plumes, and the absence of any operculum, would point at once to the genus *Protula** as that to which this species belongs, were it not for two most remarkable peculiarities of its organization, which, so far as we know at present, are to be found in no *Protula;* and one of them in no other tubicolar annelid.

These peculiarities are, in the first place, that this species undergoes *fissiparous multiplication ;* and, in the second, that it is *hermaphrodite*—the male and female reproductive elements being, unequivocally, developed in the same individual.

So far as I am aware, the process of fissiparous multiplication has hitherto been observed in only one family among the errant annelids, the *Syllidea* (of Grube); in only one family among the *Scoleidæ* (*Hirudinidæ* and *Lumbricidæ*), that of the *Naidea*,—and in only one genus among the tubicolar annelids, *Filograna.*

---

* On consulting the original description of *Filograna*—a genus to which the form of the Vermidom of this species would at first induce one to refer it, its affinities therewith appear evident; but whether there is any real difference between Filograna and Protula is a question for further consideration.

Hermaphrodism has hitherto been observed in no errant or tubicolar annelid.* Indeed the author to whom we are indebted for the most beautiful researches into annelid organization extant, M. de Quatrefages, thus concludes his elaborate memoir on the nervous system of the annelida :—

"We must then seek elsewhere (than in the nervous system) the characteristics on which to base the divisions which are necessitated by the great extent of this group, and the multiplicity of types which it embraces. Now, as an anatomical character, there is nothing more distinct and well marked than the union or separation of the sexes in the same individual. These differences of organization, besides, indicate profound physiological distinctions, which have long been justly appreciated by botanists. I am, therefore, more and more inclined to believe that the distinction of the annelids (Vers) into monœcious and diœcious ought to be adopted in science."†

In arriving at this conclusion, M. de Quatrefages was, of course, only furnishing additional evidence for the justice of that division of the annelids into the *Annèlides* proper, characterized by the separation of their sexes—and the *Scoléides*, characterized by their hermaphrodism—which was first established by M. Milne-Edwards, and which has been very generally received.

However, on a careful survey of the whole class of worms, many facts come to light which throw considerable doubt on the propriety of raising unisexuality or hermaphrodism into distinctive characters of large groups. We have hermaphrodite *Rotifera*, and unisexual *Rotifera*. The *Nemertidæ* and *Microstomum* are unisexual, the other *Turbellaria* hermaphrodite ; there appears to be considerable doubt as to the universality of hermaphrodism in the *Trematoda* even ; and *Echinorhynchus*, which cannot be placed very far from the *Tœniadœ* and *Distomata*, is well known to be unisexual, and

---

* See among other authorities, Frey and Leuckart, *op. cit. inf.*, p. 87, who examined Hermella, Vermilia, Fabricia, and Spirorbis, among the tubicolar annelids, with especial reference to this point.

† Types inférieurs de l'Embranchement des Annelés. Ann, des Sc. Nat. 1850.

there is therefore, perhaps, nothing so very anomalous in the discovery of a truly hermaphrodite tubicolar annelid. It is another question how far it need affect the classification to which I have alluded.

The fluctuation in the terminology of the classification of the annelids, in fact, has proceeded from the very common but always obstructive practice of giving notional instead of trivial names to incomplete groups of animals. Cuvier divided the annelids into errant, tubicolar, terricolar, &c., deriving his terminology from the habits of those with which naturalists were then acquainted; but, with the advance of knowledge, it was found that some of the *Errantia* inhabit tubes, while one main division of the "*Terricola*" consists of aquatic worms; and thus these notional terms, instead of aiding the memory as they were intended to do, served simply to originate and propagate erroneous conceptions. There can be no doubt that the divisions established by Cuvier are essentially natural, and had he devised some happily unintelligible Grecism, instead of the names which he actually adopted, they would have stood, their definitions altering with the progress of knowledge, until this day.

The divisions proposed by M. Milne-Edwards possess exactly the qualification which is here wanting. *Annélides* and *Scoléides* may mean anything, and, as names of groups, may very conveniently remain, even if it should be found necessary to remodel the whole definition which was primarily assigned to them. It appears to me, therefore, that if the statements which follow be confirmed, they will lead, not to an alteration or subdivision of the group of *Annelides*, but to a widening of its definition so as to include hermaphrodite forms; or perhaps it would be better to admit that owing to the imperfection of our knowledge, we have not yet a *definition* of either *Annélides* or *Scoléides* at all, but that we must arrange under the former head all those worms which resemble the errant and tubicolar sea worms more than anything else, while those which resemble the land and fresh water worms must fall under the latter category. If, from the great division of the *Annulosa*, we take away those animals which are characterized by the possession of one or more of the following characters—1. Articulated appendages. 2. Such appendages modified into jaws around the mouth. 3. A true heart in communication with the perivisceral cavity: that is, the Insecta, Myriapoda, Arachnida, and Crustacea—we have left a large division of the animal kingdom, to which the old term of *Vermes* might well be appropriated, had it not been already used in so many significations. For this division, whose members are united by a marked community of structure and development, and which includes the *Annelida* of Cuvier and a large section of his *Radiata*, viz., the *Entozoa*, the *Rotifera*, and the *Echinodermata*, I have elsewhere proposed the name of *Annuloida*, a term parallel to that very useful one of *Molluscoida* (*Molluscoides*), invented by Milne-Edwards for the *Polyzoa* and *Ascidians*.*

If it be remembered that it is only within the last few years that the structure and development of these *Annuloida*—which present extraordinary difficulties to the investigator—have been made the subjects of thorough and complete examination, it will not be a matter of surprise that, at present, the subordinate division of the group must be effected more by reference to types than by exact definition. Of course this is still more the case with the smaller sub-divisions; and until much more light has been thrown on these most interesting but most perplexing creatures, I think it would be well to understand the existing classes and orders to be purely conventional and artificial. For my own part, I doubt greatly whether any well-marked natural demarcation can, at present, be drawn between the *Annelida* (M. E.) and the *Scoleidæ*, or between these and the *Entozoa*; or, again, between the latter, the *Turbellaria*, and the *Rotifera*; or, once more, between the *Annelida* and the *Echinodermata;* though I have little doubt that the progress of inquiry will tend here, as elsewhere, to eliminate osculant forms, and to substitute definitions for types.

---

* In writing this passage it escaped my memory that the very same division had been long ago proposed by Milne-Edwards himself:

"Je crois qu'il faudrait diviser cet embranchement (Les Articulés) en deux groupes principaux, l'un les articulés a pieds articulés, et l'autre les annèlides, les Helminthes, les Rotateurs, &c., serie à laquelle on pourrait donner le nom vulgaire des Vers." Sur la circulation dans les Annèlides. Ann. des Sc. Nat.. 1838, p. 194.

Not only does it appear to me that, under these circumstances, it is inexpedient to create new sectional terms; but until a more extended and careful examination of the tubicolar annelides shall have been made with reference to these very points, I do not think it is worth while even to found a new genus for the form I am about to describe, as it possesses all the essential characters of *Protula*. Specifically, however, it appears to be distinct from all forms of *Protula* hitherto described, and I therefore propose to call it *Protula Dysteri*, after my friend Mr Dyster of Tenby, in whose society it was discovered, and from whom I hope some day to see good work in this branch of science.

I have already described the vermidom of this species, and I now therefore pass to the details of the organization of the animal itself. *Protula Dysteri* (fig. 3) possesses a very elongated body, which may be conveniently divided into a cephalic, a thoracic, an abdominal, and a caudal portion.

The cephalic portion (fig. 3, *e*) can hardly be said to constitute a distinct head, for the oral aperture, which is wide and funnel-shaped, is terminal. The dorsal margin of the oral aperture is formed by a prominent rounded lobe, beneath which are two richly-ciliated, short filaments, which adhere to the base of the branchial plumes, and might be regarded either as their lowest pinnules, or perhaps, more properly, as tentacles analogous to the operculigerous tentacles of the Serpulæ. On the ventral side the margin is deeply incised, so that a rounded fissure, bounded by two lips, lies beneath and leads into the oral cavity. From each side of the head springs a distinct branchial plume, whose peduncle immediately divides into four branches. These are beset with a double series of short filiform pinnules, the origins of each series alternating with those of the other. The termination of each branch is somewhat clavate, and when expanded the eight branches are usually gracefully incurved towards one another, the whole having not a little the aspect of a Comatula.*

The thoracic portion of the body (fig. 3, *e f*) is short, but wide and somewhat flattened. It is produced laterally into nine

---

* It is worthy of note, how very crinoid the branchial plumes would be if their skeleton were calcified instead of simply cartilaginous.

pairs of close-set, double pedal processes. The lower portion of each process forms a mere transverse ridge, beset with the peculiar hooks to be described by and by; the upper process, on the other hand, is conical, and is provided with elongated setæ. The most striking feature of the thorax, however, consists in the peculiar membranous expansion, (*b*) which, arising as a ridge upon each side of what might be termed the nuchal surface of the animal, and attached to the sides of the thorax, above the bases of the feet, runs down to terminate on the ventral surface, behind the last pair of thoracic appendages. From this origin it extends as a wide free membrane beyond the setæ, forming an elegant collar around the head, on whose ventral surface the expansions of each side unite, and form a wide reflexed lobe (fig. 4, *g*), while posteriorly they remain separate. To the thorax succeeds what may be called the abdomen, which is much longer than the other regions of the body; and is, besides, distinguished from them by the imperfect development of the feet, and the paucity of the setæ and hooks. In this, and in the caudal portion of the body, the relative position of the hooks and setæ is the reverse of what it is in the thorax, the former being superior, and the latter inferior. *

The caudal portion of the body is short, and wider than the abdomen. Its rings are close-set, with well-developed hooks and setæ, and it is terminated by two conical papillæ between which the anus is situated. There are not less than 50 rings in the whole body. Cilia could be detected in active motion on many parts of the external surface, on the bases of the feet, on the rudimental tentacles, and scattered in tufts over the whole surface of the thoracic expansions.

Having thus sketched its external character, I will now pass to the minuter features presented by the organization of the animal.

Branchial plumes.—The principal mass of these organs is formed by a clear, firm, supporting axis, so marked transversely as very closely to resemble the *chorda* of an *Amphioxus*. The lower end of this axis terminates by a somewhat pointed ex-

---

* According to Grube, this is the case in all the Serpulacea. See his most excellent work—“ Die Familien der Anneliden.” 1851.

tremity, which lies in immediate proximity to the œsophagus (fig. 4), and receives the insertion of the lateral longitudinal muscles of the body. Superiorly, as has already been said, the axis divides into four branches, one of which enters the stem of each branchia and forms its skeleton and support, sending lateral processes into each of the pinnules. These, however, are much more delicate, and are composed of oblong particles set end to end; somewhat like the axis of the tail of an Ascidian larva. All this branchial skeleton, as one might term it, is invested by a continuation of the general parietes of the body, which adheres closely to the outer side of the stem and pinnules, but leaves a space on their inner side. In this space lies the so-called "blood"-vessel, with its green contents. It does not fill the space, but lies loosely in it; the interval between it and the walls of the filament being, I suppose, in continuity with the perivisceral cavity.*

The whole of the internal surface of the branchiæ is provided with long, close-set, vibratile cilia, while nothing of the sort is visible externally. The end of the stem has a very peculiar structure. It is somewhat enlarged by the development within its walls of a number of elongated granular masses of about $\frac{1}{1000}$ inch in length, entirely made up of very minute, strongly refracting granules, which, when pressed out, become rapidly diffused and dissolved in the surrounding water. These bodies were not confined to the ends of the branchial stems, but similar aggregations existed at the ends of many of the pinnules, and were also very regularly developed in little elevations seated upon the sides of the stem in front of the base of each pinnule.†

Alimentary Canal.—The œsophagus leads into a pyriform, more or less marked, dilatation or crop, provided with thicker

---

* The skeleton of the branchiæ of the Serpulacea has been well and carefully described by De Quatrefages in his valuable memoir "Sur la circulation des Annelides," Annales des Sciences Naturelles, 1850; and that of Sabella unispira by Grube, so long ago as 1838. See his memoirs "Zur Anat. und Physiologie der Kiemenwurmer." 1838.

† Are the peculiar rounded whitish granular patches which occupy a similar position on the arms of Comatula of a corresponding nature, or are these really testes? I have never been able to find developed spermatozoa in them, nor anywhere else in Comatula.

walls than the remainder of the alimentary canal (fig. 5). The crop communicates by a constricted portion with a wide stomach, whose walls are strongly tinged by deep brown granules. This passes into a narrow intestine, which widens in the caudal region into a sort of rectum, opening externally, between the terminal papillæ, by a richly-ciliated anus.

In every segment the intestine was united to the parietes by delicate transverse membranous dissepiments, forming partitions across the perivisceral cavity, and thus dividing it into a series of chambers, which, so far as I could observe, did not communicate with one another, though it would be unsafe absolutely to affirm this.

"*Vascular*" *System.*—The so-called "blood"-vessels* of the Annelida were represented, in the present case, by lateral contractile vessels which ran upon each side of the intestine, and gave off transverse branches on to the dissepiments, from which twigs proceeded dorsally and ventrally.

The dimensions of these lateral vessels varied considerably; sometimes they were comparatively narrow, but in other instances so wide as to appear to form a complete sheath around the intestine. They contained a deep green, clear fluid, totally without corpuscles or solid elements of any kind, while they themselves, when empty, were usually quite colourless; but I would draw attention to the curious fact, which I have also observed in other annelids, that in the anterior part of their course they occasionally present bright green, granular particles, imbedded in, and adhering to, their outer surface.

The opacity of the anterior end of the animal, resulting from the quantity of deep red pigment, prevented any very

---

* At the last meeting of the British Association (September 1854), I ventured to propound the theory that what are commonly called the blood-vessels of the Annelida are not "blood"-vessels at all; that is, that these vessels, and the fluid which they contain, are not the homologues of the blood-vessels and blood of Vertebrata, Mollusca, and Articulata, the latter being represented in annelids by the perivisceral cavity and its contained fluid, whose anatomical and physiological importance have been so excellently and exhaustively developed by De Quatrefages. See his researches on the Annelids, and more particularly his memoir "Sur la cavité generale du corps des Invertebrès." It is to be hoped that M. de Quatrefages understands that instructed Englishmen do not countenance the unwarrantable attempts that have been made to depreciate his merits in this country.

certain observation of the manner in which these vessels terminate there. I am inclined to think, however, that they open into a circular vessel, from which the branchial vessels arise.

It was no less difficult, in an adult specimen, to determine whether a ventral vessel existed or not; but in a young form, I saw such a vessel communicating with the inferior transverse branches, and distinctly contracting. It was superficial to the ciliated canal immediately to be described.

Of a dorsal vessel I could find no trace. The final ramuscules of the superior transverse branches of the lateral trunks were found, whenever they could be distinctly observed, to terminate *cæcally*. There could be no question whatever, that these cæcal ends were the natural terminations of the ramuscules, as the animal under observation had been subjected to no violence, and was viewed by transmitted light. I am the more particular in insisting upon this point, as one might very readily be led, in dissecting annelids, to suppose that cæcal terminations of the vessels are much more frequent than they really are. Their vessels, in fact, possess, in a very high degree, that tendency to contract when torn, which is so well known in the arteries of the higher animals. And if under the simple microscope the vessels of an Eunice or Nereid be deliberately pulled asunder, it is most curious to observe how very little of the contained fluid pours out, and how smooth and round the torn ends immediately become. In our *Protula*, however, the mode of examination was such as to preclude all chance of error from this source; and I have besides fully confirmed the fact of this mode of termination,* in the singular and beautiful genus *Chloræma*, which has the advantage of great transparency. In this animal it is easy to observe that, though many of the ultimate branches of the vessels anastomose, and thus give rise to a network, yet that there are also many branches of no inconsiderable dimensions, which terminate in cæcal extremities. Such vessels may be frequently observed coming off from the transverse trunk and hanging freely into the peri-

---

* This cæcal termination of the vessels appears to reach its greatest development in the Scoleid genera, Euaxes and Lumbriculus, in which a vessel arises in each segment from the dorsal trunk, and shortly divides into many cæcal ramuscules. See Siebold. Vegleichende Anatomie, p. 212.

visceral cavity, attached only by a few delicate threads of connective tissue, to the parietes. It is most curious to watch the regular contractions of these pendent vessels, their momentary emptying, and their subsequent distention and erection by the returning wave of fluid. And in considering the nature of this remarkable system of vessels, it is most important to note that we have here, at any rate, no circulation, but a mere backward and forward undulation.*

Ciliated Canal.—A clear, longitudinal, very narrow ($\frac{1}{1400}$ to $\frac{1}{3300}$ inch) canal (fig. 6, *a*) may be observed extending along the ventral surface of the intestine in the middle line, from the anus, where it appeared to me to open, as far as the brown dilated stomach, when it either stopped or became so obscured as to be no further traceable. The canal had well-marked walls with a double contour, which sometimes appeared curiously broken; and contained, set along its dorsal wall, one to four longitudinal series of cilia (fig. 9). These were placed at regular intervals, and worked together, as if they were pulled by a common string. In young specimens there was only one cilium in each row, but in the older ones I saw as many as four in each transverse line. Has this enigmatical canal anything to do with the 'typhlosole' of the earthworm ?

On the dorsal surface of the head a longitudinal canal, which sometimes appears to be ciliated, was visible at *b* (fig. 3); posteriorly it divided into two branches which dilated into granular cæca, arranged in a kind of festoon in the first segment of the thorax.

The coloration of this part of the body prevented me from determining whether this canal opened externally or into the œsophagus, and also whether it was in any way connected with the ventral ciliated canal,—both of them points of much interest.

However this may be, these sacs are clearly homologous with the curious sacs which have been described in *Chlorœma*, and perhaps with the sacs opening externally, which are found in the anterior segment of *Pectinaria*.

---

* The general contractility of the vessels of the annelids has already been pointed out by De Quatrefages. Siebold doubts the existence of a regular circulation in the majority of the *Annelida*. *Op. cit.*, p. 210.

I may mention here that ciliated organs, possibly homologous with these, and with the lateral convoluted canals of the *Lumbricidæ* and *Hirudinidæ* are by no means uncommon among the *Annelida Errantia,* and may be observed in *Phyllodoce;* it requires care however to discover them.

Nervous system.—On this head the result of my examinations was exceedingly unsatisfactory, as I could assure myself of the existence of only two oval ganglia, one on each side of the œsophagus, each of which presented a dark pigment mass (eyespot ?) on its anterior extremity.

Reproductive elements.—Protula Dysteri can hardly be said to possess special reproductive organs, the reproductive elements, viz., ova and spermatozoa, being developed as it were accidentally from the walls of the perivisceral cavity, by the fluid contained in which (whose nature and importance M. de Quatrefages has so well pointed out) they are bathed, and supplied with nutritive materials. It appeared to me that the spermatozoa or ova took their origin in granular thickenings of that portion of the face of the dissepiments which is traversed by the transverse vessel, becoming detached thence, and floating freely in the perivisceral fluid, as they attained their full development. *

The youngest spermatozoa were minute spherules, of not more than $\frac{1}{5000}$ of an inch in diameter, aggregated together into irregular masses (fig. 11). In a more advanced state a very fine short and delicate filament could be observed springing from one side of this body. By degrees the spherule became elliptical, and narrowing *pari passu* with the elongation and thickening of the filament, the ultimate result was a spermatozoon, such as that represented in fig. 11, with a subcylindrical slightly pointed head of $\frac{1}{3500}$ of an inch in diameter, and a very long actively-undulating tail.

The ova are, at first, very small, not more than $\frac{1}{4000}$ of an inch in diameter, and possess a relatively very large, clear space, representing the germinal vesicles, containing a minute

---

* Frey and Leuckart (Zool. Untersuchungen, p. 88) assert that the generative elements of the annelids are developed from a free blastema, and not from the septa only, as Krohn asserts to be the case in Alciope, and as I should, from what is stated above, be disposed to believe.

germinal spot. By degrees they increase in size to $\frac{1}{600}$ inch, with a germinal vesicle of $\frac{1}{1000}$, and a spot of $\frac{1}{3500}$, and a few granules become visible in their yelk. From this size they gradually increase to the $\frac{1}{150}$ inch in diameter, acquiring a well-marked vitellary membrane, and a dark orange-red, very coarsely granular yelk. The germinal vesicle and spot may still be rendered visible by pressure, the former having about $\frac{1}{600}$ of an inch in diameter.

When those segments of the body in which the genitalia are situated were subjected to moderate pressure, the spermatozoa made their exit at the bases of the pedal tubercles of the male segments, while the ova, just giving rise to bulgings in a corresponding position, eventually passed out in the same manner. I could not satisfactorily decide, however, whether the apertures by which the generative products passed out were natural or artificial.*

Setæ and Uncini of the Pedal Tubercles.—The general form of the pedal tubercles has already been described; it remains only, therefore, to note more particularly the form of their appendages, whether *Setæ or Uncini.* The *Setæ* (figs. 7, 8) are slender spines, about $\frac{1}{80}$ of an inch in length, consisting of a haft and a blade; the former is about six times the length of the latter, and is rounded, flattening gradually as it passes into the blade, with which it is completely continuous, though at an obtuse angle.† The blade tapers gradually to its point, and is smooth on one edge, but minutely denticulated upon the other, while delicate striæ are continued from the serrations upon the flat face of the blade.

Such is the structure of those stronger setæ which are directed forwards on each side of the head-lobe. Those of the

---

* It should be added that the genital products occupy about fourteen successive segments of the abdomen, of which the two anterior are seminiferous; the rest, ovigerous. See fig. 3.

† I am not aware of any annelid in which the setæ are really articulated. The statements of Audouin and Milne-Edwards rest, I believe, upon errors of observation, very intelligible, if one considers what microscopes were twenty years ago. How such strange perversions of fact as the figures of annelid setæ appended to Dr Williams's Report on the British Annelida, published in the Transactions of the British Association for 1851—can have arisen, it is not so easy to comprehend.

posterior segments have a similar general structure, but are more delicate.

The *uncini* (figs. 7, 8) are very small, not more than $\frac{1}{1000}$ inch in length; and it is not easy to make out their exact structure. Each, however, appears to be composed of a short implanted stem, and a blade set upon the end of this, at somewhat less than a right angle, like the claw of a hammer. The edges of this blade are minutely denticulated.

Fissiparous multiplication.—It was only a minority of the *Protulæ* which presented the aspect hitherto described; for the larger number were undergoing multiplication or prolification, by a process which can only be described as a combined fission and gemmation. The prolification takes place so as to separate all the segments of the parent behind the sixteenth, as a new zöoid; but it is not a mere process of fission, for the seventeenth segment, *i. e.*, the first of the new zöoid, undergoes a very considerable enlargement, and eventually becomes divided into the nine segments of the head and thorax, of the bud. These segments do not appear all at once, but gradually, one behind the other. The intestinal canal of the stock and of the bud are at first perfectly continuous, but the peri-intestinal cavity of the bud is completely filled with a mass of red granules. These would seem in some way to subserve the nutrition of the young animal; for in some free zöoids, apparently fully formed, all but the development of genitalia, the caudal segments were full of these orange granules, while no trace of them was to be found anteriorly.*

It is very interesting to note the manner in which the branchial plumes are developed, as it closely corresponds with what Milne-Edwards describes in *Terebella*. Each plume appears at first as a quadrate palmate process of the dorsal side of the first segment; and the divisions representing the stems of the future branchiæ are at first mere processes,—perfectly simple tubes, which do not even present annulations.

Several modes of prolification are already known to exist among the annelids. The one long since described by O. F. Müller, as one of the methods of multiplication of *Nais*, and

---

* Sars gives an account of the prolification of Filograna implexa, similar in all essential points. See his Fauna littoralis, &c., pp. 88-9.

more lately by Quatrefages as occurring in *Syllis prolifera* is very nearly simple fission, the animal dividing near its middle, and the under half, before separation, only putting forth, as buds, those appendages which are characteristic of the head.

Secondly, Milne-Edwards has described in *Myriadina* a prolification by a sort of continuous budding between the anal and the penultimate segment. A new ring is produced behind the penultimate segment, and this enlarging gives rise to a new ring posteriorly, and so on until the bud attains its full length.

It would seem possible that the second mode of prolification in *Nais,* described by O. F. Müller, is in reality the same as this, though he describes the new growth as entirely resulting from the excessive development of the anal segment.

Thirdly, M. Schulze, an excellent observer, has described a third very singular mode of prolification in *Nais,* whence the long chains of zooids occasionally observed arise. For when, by the fissive process the *Nais* is divided into an anterior and posterior zooid, the last segment of the former greatly enlarges, becomes divided into segments, and the anterior of these becoming a head, a new zooid is formed between the previously existing ones; this process is repeated in what was the penultimate, but is now the ultimate segment of the anterior zooid; and, again, in the anti-penultimate, so that at least a long string of zooids is formed, each of which, except the last, is produced from a single segment.

Fourthly, According to Frey and Leuckart, whose observations have been confirmed by Krohn (Wieg. Archiv., 1852), *Autolytus prolifer* multiplies in a somewhat similar way, but instead of each new interposed zooid being formed at the expense of a fresh segment of the anterior zooid—it is produced by the metamorphosis of a bud, or rather of a mass of blastema the equivalent of a bud, developed from the under extremity of the last segment of the anterior zooid.

Supposing further observation to confirm the distinctness of all these modes of prolification, they might be classified according to the amount of the already formed parental organism which enters into the produced zooid.

1. All the segments of the latter were segments of the former, the new products being merely cephalic organs.

2. None of the segments of the produced zooid belonged to the parent zooid, but the former is a metamorphosis of a whole segment of the latter.

3. None of the segments of the produced zooid belonged to the parent zooid, and the former contains hardly any of the primitive substance of the latter, being developed by germination from its last segment.

It is clear that the prolification of *Protula Dysteri* will come under none of these categories; but is a combination of the first and second methods. The abdomen of the produced zooid is a mere fissive product of the parent, but its thorax is the result of the metamorphosis of a single segment of the parent into many segments.

Quatrefages endeavoured to show that the relation of the produced zooids of *Syllis* to the anterior zooid was that of an "alternation of generation," the former alone developing sexual products. Krohn has however proved that no such relation exists in this case; but on the other hand he brings forward good evidence to demonstrate that the posterior zooids of *Autolytus prolifer* really *are* generative zooids, and alone develop the reproductive elements. The male zooids in this case are widely different from the gemmiparous zooid; so different, in fact, that they were regarded by O. F. Müller as belonging to a distinct species.

I sought carefully for evidence of any such "alternation" in *Protula Dysteri*, but the result was to convince myself that nothing of the kind exists.

The generative products may indeed almost always be detected, though the ova are very small and indistinct, in the anterior zooid of any still unseparated pair; and it is therefore clear that the gemmiparous zooid is not asexual, the invariable rule where that separation of the individual into asexual and sexual zooids, which constitutes the so-called "alternation of generations," really exists.

Fig. 6

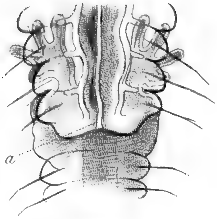
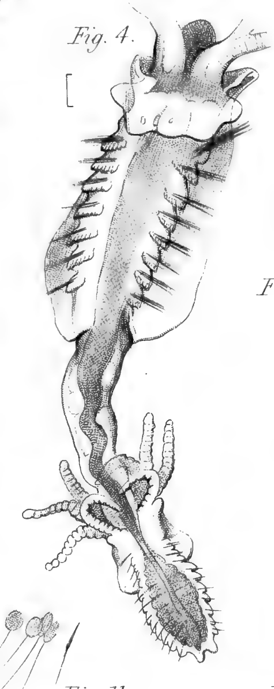


Fig. 1.

Fig. 2.

Fig. 4.



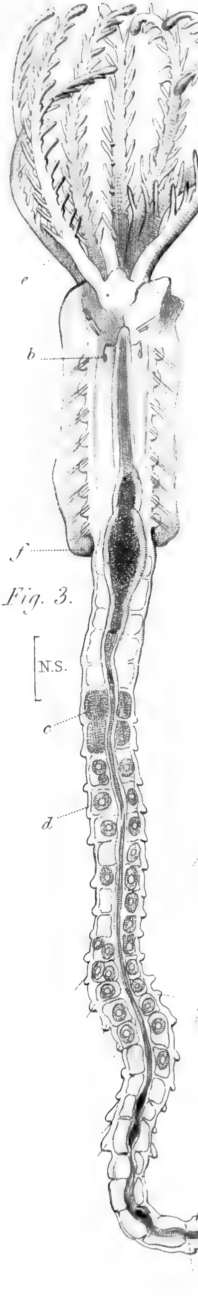


Fig. 3.

Fig. 5.

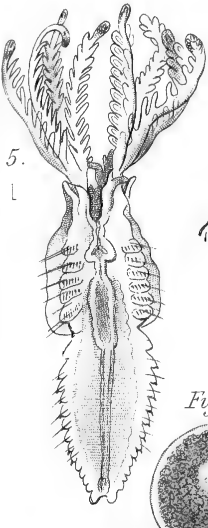
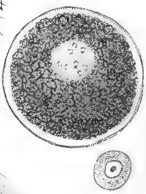


Fig. 9.

Fig. 10.



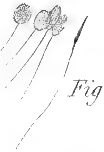


Fig. 11.

Fig. 7.

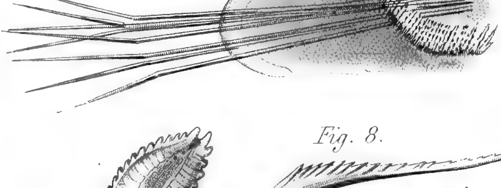


Fig. 8.

ad nat. del. T.H.H.

J. Basire sc.

Description of Figures, Pl. I.

Fig. 1. Vermidom of *Protula Dysteri.*
2. Single calcareous tube with the worm protruded and expanded.
3. An adult *Protula* extracted from its case, *c.* branchia, *c.* testes, *d.* ova,—(dorsal view).
4. A *Protula* undergoing prolification (central view).
5. The produced zooid just set free.
6. Junction of parent and derivative zooids (ventral view), *a.* ciliated canal.
7. Pedal tubercle.
8. *Setœ and Uncini.*
9. Ciliated canal, greatly magnified.
10. Ova—young and completely developed.
11. Spermatozoa—young and completely developed.

On the Artificial Preparation of Sea Water for the Aquarium. By GEORGE WILSON, M.D., F.R.S.E., Lecturer on Chemistry.*

In an interesting communication contained in the "Annals of Natural History, for July 1854 (p. 65), Mr Gosse has recorded the results of an important experiment on the possibility of artificially preparing sea water for Marine Vivaria. Guiding himself by Schweitzer's analysis of the water off Brighton, and excluding the less abundant ingredients, he employed chloride of sodium, sulphate of magnesia, chloride of magnesium, and chloride of potassium,† which were dissolved in a suitable quantity of water. In April last various species of marine plants and animals were introduced into this imitation sea water, and as during a period of six weeks they "throve and flourished from day to day, manifesting the highest health and vigour," Mr Gosse draws the very natural conclusion,

---

* Read to the Chemical Section of the British Association, September 1854.

† The following are Mr Gosse's exact directions:—Common table salt, 3½ ounces; Epsom salts, ¼ ounce; chloride of magnesium, 200 grains troy; chloride of potassium, 40 grains troy. To these salts a little less than four quarts of water were added.

"that the experiment of manufacturing sea water for the aquarium has been perfectly successful."

In spite of this success, however, there are cogent reasons for believing that sea water made according to the recipe given above, would fail to maintain for any length of time either plants or animals in health and vigour.

Mr Gosse's sea water differs from that of the ocean in not containing several ingredients which must be regarded as essential to the growth of sea plants, and still more of sea animals. It contains only such of the constituents of the ocean as are soluble in *pure* water, and only some of these. Thus, although it may be difficult or even impossible to detect in considerable volumes of natural sea water, carbonate of lime, sulphate of lime, phosphate of lime, fluoride of calcium, and silica, all of these as well as oxide of iron are procured in manifest quantity by evaporating sea water to dryness, as I have many times ascertained by analysing the hard crusts from the boilers of steam-ships, sailing in the Atlantic and German oceans, and in the Mediterranean and other seas. The sulphate of lime and fluoride of calcium are soluble in pure water, and the carbonate and phosphate of lime are kept in solution by carbonic acid. The silica is either held simply in solution, or occurs as a soluble alkaline silicate.

Now it is plain that marine animals (to restrict ourselves to them) must derive all their constituents, directly or indirectly, from the medium in which they live; and the law does not appear to admit of any question, that whatever substances are *invariably* found in the structures of animals, must be essential to their healthy development, and this whether the substance is present in large or small quantity, provided it is invariably present. Thus, to take one example, we find fluoride of calcium, not isolated in one minute portion of an animal's body, but built up along with phosphate of lime wherever that occurs. It seems a dangerous rule to go by, that because the quantity of fluoride is much smaller than that of phosphate, the fluoride may be omitted altogether. We might as well, I apprehend, in erecting a house, dispense with mortar, because the quantity used in building is very small, compared in weight or bulk, with that of the stones it binds together.

Seeing, however, that the internal and external skeletons, habitations, or other solid appendages of many of the animals kept alive in aquaria, consist of carbonate of lime, along with some phosphate of lime, and a little fluoride of calcium, whilst others consist of silica—those substances besides iron must be contained in the water in which these creatures dwell.

Again, to refer to sea plants, Mr Gosse excludes from his sea water, soluble bromides, and, as appears, also iodides, because they occur in the ocean in small quantities. Yet it is quite certain that many sea-weeds concentrate within themselves much iodine as well as a little bromine, and both, but especially the former, must be held to be serviceable to those plants. It may be added, that although no minute inquiry into the matter has been made, both iodine and bromine occur in the organs of sea animals, for example, in the liver of the cod; and it is impossible to believe that such powerful remedial agents, can be without an influence on the health of the animals receiving them. Iodides and bromides, therefore, should be present in the imitation sea-water.

Nor would there be any difficulty in supplying the desiderata indicated. As calcareous phosphates, carbonates, and fluorides occur together in shells, corals, and many limestones, and in the proportion in which sea animals require them, the arrangement of fragments of such calcareous bodies at the bottom of the aquarium would suffice;—for the carbonic acid produced by the animals within it would slowly dissolve the lime-salts as they were needed.

Pieces of felspar or of any of the trap rocks containing alkaline silicates would in the same circumstances furnish silica. It would not probably be requisite to make a deliberate addition of sulphate of lime, as the sulphate of magnesia and the calcareous fragments would supply its elements. If it were thought necessary to add it, a solution, containing about a grain of sulphate of lime to the ounce of water, can be easily prepared by shaking the latter with some burned stucco powder, and of this a measured quantity could be added to the contents of the aquarium. There would be no difficulty in supplying bromides and iodides, as the bromide and iodide of potassium may be procured from any druggist.

It is of course quite possible that in a single aquarium the death of a certain portion of the animals might furnish calcareous salts or silica for the skeletons of their survivors,* and in like manner, the death of a given number of the plants might liberate iodides and bromides for the remainder; but the object of those who maintain aquaria, I presume to be, the rendering as certain as possible the vigorous development of all its living contents, and this could only be secured by some such arrangement as I have proposed.

As aquaria are now attracting much attention among naturalists, I would suggest the desirableness of some of them trying how long animals will live in sea water made strictly after Mr Gosse's recipe, and without any calcareous or silicious fragments at the bottom of the vivaria. Those observers also who record their success with artificial sea water should be as careful in stating the chemical composition of the stony fragments laid at the bottom, as of the water employed in filling their aquaria. In their aquarian experiments hitherto, naturalists have guided themselves chiefly by the results of the chemist's analyses of sea-water. But these supply but one-half of the requisite data: the naturalist should have equally regarded the analyses of marine plants and animals; for if any substance is invariably found in them, it must as invariably be furnished in the liquid or solid contents of the aquarium. The minuteness of quantity in which particular ingredients occur in living organisms can only be a reason for furnishing them in minute quantity not for omitting them altogether.

---

* Mr Gosse observes that carbonate of lime "*might* be found in sufficient abundance in the fragments of shell, coral, and calcareous algæ thrown in to make the bottom of the aquarium" but he nevertheless refers to it as one of those substances which he thought he "might neglect from the minuteness of their quantities." The practice here corrects the error of the precept, for the calcareous fragments would furnish not only carbonate of lime, but salts of magnesia, as well as phosphate of lime and fluoride of calcium.

The late Professor Edward Forbes.

We need not now endeavour to give expression to a grief so deeply felt and universally diffused, as that occasioned by the sudden and disastrous death of this distinguished naturalist. Ours is the loss, and, we doubt not, his the gain. The disadvantages, both of a personal nature to his private friends, and of a more public kind to the community at large, are inexpressible and irremediable. If all hearts are still saddened by this heavy and unlooked-for calamity,—if even those who knew him not, or had but a faint idea of his surpassing powers,—are impressed with so deep a sense of this bereavement,—how much more must it weigh down the spirits, almost deaden the hopes, of those who were associated in his labours, but who felt their labours lightened by their rejoicing confidence in such a companion and coadjutor. Viewing the loss as amounting to a national misfortune, not to be measured merely by the sudden sorrow produced among ourselves by its unexpected occurrence, amid the first upraising of so many fresh and sanguine hopes, we shall not dwell upon its great disadvantage to this Journal, the management of which he was about to undertake, with all his well-known and unfailing zeal, as Editor of the Natural History department, in its various branches.

As it might truly be said of Professor Edward Forbes "nil tetigit quod non ornavit," so, under his fostering care and skilful hand, whatever of barren and unfruitful might have unavoidably crept in upon our management of later years would have been corrected or expelled, and new life and vigour interfused. But having been honoured with his confidence, we shall consider the increased responsibilities thrown upon us by his disastrous death, as so many pledges to the Public, that this Journal, to which he so fondly desired to devote himself, shall be conducted, if not with the same talent, at least in the same tone and temper, as distinguished every procedure of him whom we deplore.

We shall here present a brief and most inadequate record of his life and labours, drawn from our own knowledge and recollections, aided by reference to some friendly and affectionate reminiscences which have already appeared in several of the literary and other Journals.*

Professor Edward Forbes, so recently, and with such universal satisfaction, appointed to the chair of Natural History in our university, died at Wardie, near Edinburgh, on the evening of Saturday, the 18th of November 1854, in the fortieth year of his age, leaving a widow, and a son and daughter still in infancy, to mourn and suffer from his loss. The certainty of his appointment had been long foreseen, and was looked forward to as an event likely to give a fresh impulse among us to the study of natural science in every department. He had received his scientific education here,—had here formed several of his strongest and most enduring friendships; and his early celebrity, and continuing increase of fame, had been nowhere observed with more pride and pleasure, than among those who had started with him in the race of life. When he returned to Edinburgh, it was to the "old familiar faces," changed, no doubt, from youth to manhood, but rejoicing all the more to receive again in social and scientific union one between whom and them not even the shadow of a passing cloud had been ever interposed. It is indeed worthy of record, that among his earliest and most endeared associates, he was welcomed back by such

---

* The death of Professor Edward Forbes has been feelingly and faithfully recorded at considerable length, and apparently from intimate personal acquaintance, in the Athenæum, Literary Gazette, Spectator, and Gardener's Chronicle; as well as in the Witness, and other Edinburgh newspapers. We are happy, however, to announce that a much more ample and satisfying memoir of his life and writings has been undertaken, with the concurrence of his literary executor, Mr Austen, by a kindred spirit, and early friend, Dr George Wilson, F.R.S.E., already so well known as a biographer, from his lives of Cavendish and Dr John Reid. We had hoped to present this memoir in the April number of our Journal, and have therefore restricted ourselves, in the meantime, to what we fear our readers may regard as by no means a satisfactory exhibition and estimate of the Professor's personal and scientific attainments. But, in deference to the wishes of those by whose feelings it is a pleasure, no less than a duty to be guided, it has been decided that the extended biographical memoir shall form a separate volume, probably introductory to a collected series of Professor Edward Forbes' works.

men as Mr John Goodsir, Mr James Syme, Mr James Miller, Dr J. Y. Simpson, Dr J. H. Balfour, Dr J. H. Bennett, and others, already professors in that same university within the walls of which, as youthful companions, their mutual friendship had commenced,—a friendship unbroken but by death.

Edward Forbes, of Scottish extraction, was born in the Isle of Man, on the 12th day of February 1815. We have heard himself say, that had he made the attempt to define the period when the love of natural history first arose as the day-star in his heart, he must have searched back into the dim and distant recollections of his earliest childhood. This peculiar propensity, or rather passion, must have been in-bred, and all his own; for it is understood that no individual of his family, nor even of his acquaintanceship, had the slightest taste for scientific studies. So this surpassing love of natural history must have been either born with him, or speedily and spontaneously generated in his brains.

His first printed guide-book was one of the driest,—Turton's English Edition of the *Systema Naturæ* of Linnæus; and we know, on his own authority, that by the time he was *seven years of age*, he had formed a small but tolerably well arranged museum of his own. Next, though still in very early life, came the perusal of Buckland's *Reliquiæ Diluvianæ*, Parkinson's *Organic Remains*, and Conybeare's *Geology of England*,—all rather difficult reading for a boy, and possibly rather wrestled with than fully understood. However, there is nothing so good as a high standard in the intellectual struggles of youth, as difficulties ere long spontaneously unfold themselves, and become smooth and shapely, just as the wings of the butterfly enlarge and brighten, when the hardened coating of the chrysalis is cast away. Neither is there anything so bad as bringing all early instruction down to a level with the limited understanding of childhood. There are few really good books which even full-grown men completely comprehend; but this, though an argument against the capacity of the readers, is surely none against the excellence of the books. Those above named, however, when he was not more than twelve years of age, inspired Edward Forbes with a warm and abiding love of Geology. At this period also, it may be stated as a remarkable, perhaps unprecedented fact, that he compiled a Manual of British Natural History in *all* its departments,—a youthful labour, a reference to which, we know, he afterwards found serviceable up almost to his close of life.

At sixteen he visited London; and while there, was chiefly occupied by the study of the art of drawing, under Sasse, a celebrated trainer for the Royal Academy in those days. The careful practice of drawing in outline from the antique, which he then acquired, was of advantage to him for ever after in his zoological pursuits and publications. About a year after this, he came to Edinburgh, and entered the medical classes, as the best course of initial and elementary study in relation to those departments of science to which he had even thus early determined to dedicate his life. He became at once the friend and pupil of Professor Jameson; and from that period till he found himself his successor (how much we mourn the brief survival!) he frequently referred, with grateful acknowledgment, to the benefit he had reaped from his scientific instruction, and friendly counsel. In the summer of the ensuing year he first endeavoured to apply practically the knowledge he had now acquired, by making an exploration of a part of Norway,—chiefly with a view to the mineralogy of that picturesque country. He returned with large collections, and published an account of his proceedings and observations in Loudon's Magazine (vols. viii. and ix.) under the title of "Notes of a Natural History Tour in Norway,"—being his first contributions to science. At nearly the same period, and in the same work, he printed his earliest papers on submarine researches,—"Records of the results of Dredging,"—for which he became eventually so noted, having, in fact, commenced in his sixteenth year those remarkable observations by means of the dredge, with the accurate *register of depths*, which, it is well known and admitted, have thrown an entirely new light upon the geographical distribution of marine life. We need not here say how amply he has filled, even to overflowing, the measure of that early promise. He has far transcended all others in the importance and extent of his submarine researches in the British Seas, as well as in those of Greece and Asia Minor.

He thus pursued his studies with great intensity of thought and application, yet with so much of the buoyant light-heartedness of youth, as no doubt to draw many very worthy common-place people into the belief that he was making no particular progress in his pursuits, and had too much of the unconstrained, it might almost seem, unacademical, spirit of the German "Burschen" in his general bearing and mode of life. But the result more than justified the hopes and expectations of those who augured, because they knew of, better things. He did not confine himself to scientific pursuits, but mingled with them many miscellaneous literary exercises, thus strengthening and enlarging his intellectual faculties, and fitting himself all the more to take eventual advantage of those points in the minds of others, to whom a discursive power, and some imaginative impulse, were required to create a tendency towards scientific studies, rather than a dry enunciation of technical details, which so often fails to affect the feelings. It is not knowledge or intellect alone that is required in science, though each is indispensable, and both are too often found wanting. There must be feeling and affection, as towards a living being, as if it formed almost an inseparable component portion of our own existence. It was thus that Edward Forbes built up all his great things on a secure foundation,—no man more cautious yet so bold,—but it was by the exercise of something akin to the imaginative faculty that he first foresaw and felt the grandeur of those general views,—such as the zones of living life, which exist not alone upon the sunny surface of the earth, but in the darksome waters far beneath it,—and which he afterwards wrought out with the patient zeal of a devoted inquirer, not less than rapid apprehension of an accomplished naturalist.* It is but

---

* The natural law above alluded to, and of which Professor Edward Forbes was the first, as he continued to be the principal exponent, is this,—that as there are great and characteristically distinct zones of animal and vegetable life, *in altitude*, as we proceed upwards on the sides of mountains, or into alpine

seldom that such a mind is born into the world, and hence our loss. If the rash hand of the fool or the maniac destroys some so-called priceless work of art,—some Portland vase, unique and unequalled in the elegance of its fair and frail proportions,—extraordinary human skill may so repair it, that ordinary human sight is deceived into the belief that it stands again before us in its first integrity, almost without a flaw; but if "the silver cords be loosed," and "the golden bowl be broken," who can re-animate the insensate form? The desolate dwelling cannot be re-entered,—the fallen column no more upraised upon the earth.

The residence of our lamented friend was continued almost uninterruptedly in Edinburgh, as his head-quarters, until 1839. We believe that 1837 formed an exceptional season, as he spent that year in Paris studying geology under Constant Prevost, mineralogy under Beaudent, and zoology under De Blainville and Geoffroy St Hilaire. During the autumn of all these busy and invaluable years he explored some interesting portion of the Continent of Europe, or beyond it, doing good service to science by a somewhat lengthened sojourn, at one time in Illyria, at another in Algiers. The results of these various visitations have been publicly recorded, as were also, about the same period, a short treatise on the Mollusca of the Irish Sea, and several papers on zoology and botany.*

---

valleys, from the sea, so there are also equally distinct and different zones of animal and vegetable life, *in depth,* as we descend (which we can only do by dredging) from the sea-shore, down the mountains, and into the great submerged and sunless valleys, of the ocean.

* See "*Malacologia Monensis,*" Edin., 1838; "On the Land and Fresh-Water Mollusca of Algiers and Bougia,"—*Annals of Nat. Hist.*, vol. ii.; "On the Distribution of Terrestrial Pulmonifera in Europe,"—*Reports of Brit. Assoc.*, 1838; "On a Shell-bank in the Irish Sea, considered zoologically and geologically,"—*Annals of Nat. Hist.*, vol. iii.; "Notice of Zoological Researches in Orkney and Shetland during the month of June 1839,"—*Reports Brit. Assoc.* 1839; "On the Asteriadæ of the Irish Seas,"—*Wernerian Memoirs,* vol. viii.; "Report on the Distribution of Pulmoniferous Mollusca in the British Islands," —*Reports Brit. Assoc.*, 1839; "On the Association of Mollusca on the British Coasts, considered with reference to Pleistocene Geology,"—*Edin. Acad. Annual,* 1840; "On a Pleistocene Tract in the Isle of Man, and the relations of its

In the winter of 1839-40, he delivered a course of lectures in Edinburgh on Zoology and Comparative Anatomy, of a strictly scientific nature, for professed working students; and he also gave at that time a course of a more popular character on Zoology, in its connection with Geology on the one hand, and Mental Philosophy on the other. Early in the year 1840, he completed his beautiful and still standard work on British Star-fish and Sea-urchins (published in 1841), adorned by not fewer than 120 accurate and highly-finished illustrations. These latter were all designed by himself; and we may here note that his artistic skill was fully and frequently employed, not only in the representation of animal forms, but in sketches both of rural and architectural scenery, and, most characteristically of all, in the vignettes and tail-pieces to his various publications, where we have humour and sentiment, gracefully and truthfully combined. This power of drawing was of incalculable advantage in his professorial career, by enabling him to exhibit to the eye many things beyond expression by the power of words. By making use of different coloured chalks, he would give most life-like sketches, not only of outer form, but of internal structure, both being in some cases of a nature so fragile, unfixed, translucent, that little or nothing could be understood regarding them, by those previously uninstructed, from the inspection of the actual subjects. But this accomplished instructor having ascertained, by the most minute and pains-taking labour, the actualities of form and substance, and having impressed them on his own mind, was able, by the combined power of a retentive memory and a skilful hand, to bring into the clearest light what was in itself invisible to common eyes, or, if visible, then incomprehensible by common intellects, till seen through the borrowed lustre of *his* understanding. Alas! it seems but as the remembrance of yesterday, that the feeling returns upon us with all its freshness, how in his recent summer course (so frankly under-

---

Fauna to that of the neighbouring Sea,"—*Reports Brit. Assoc.* 1840. We mention the preceding merely as among the more prominent of his earlier contributions, and to show how soon his determinations tended towards marine researches.

taken, and so fully accomplished) while he was demonstrating the essential nature and attributes of those almost crystalline creations from the "blue profound," of which he was himself the prime expositor, the interest of his most original descriptions was almost as it were submerged in admiration of the beautifully graceful forms which seemed to arise as if by magic from beneath his long and delicate fingers, and how a murmur of applause was not refrained from by his grateful and admiring audience,—spectators, rather they might then be called.

In April 1841, he accepted an invitation from his friend, Captain Graves, who commanded the surveying squadron in the Mediterranean, to join the "Beacon," in the capacity of naturalist, holding a nominal appointment from the Admiralty, which gave him position but no pay. He continued in the exploration of the Archipelago and of the coasts of Asia Minor, with ample and most valuable results.* The Beacon having visited the coast of Lycia in the beginning of 1842, for the purpose of conveying away the remarkable remains of antiquity discovered at Xanthus by Sir Charles Fellows, her crew were employed there in making excavations among the ruins, and preparing for the removal of the marbles; for which task, however, she proved unfitted. She therefore went back to Malta for the necessary requirements; and being expected to return to Lycia, Mr Edward Forbes and Lieutenant Spratt (having been previously joined by the Rev. Mr Daniel, an accomplished

---

* The following are a few of the important papers, the materials for which were acquired about this time. "On two remarkable Marine Invertebrata inhabiting the Ægean Sea"—*Rep. Brit. Assoc.*, 1841. "On the species Neæra (Gray) inhabiting the Ægean Sea"—*Proceedings Zool. Soc.*, xi., p. 75. "On the Radiata of the Eastern Mediterranean"—*Linn. Trans.*, xix., p. 143. "Report on the Mollusca and Radiata of the Ægean Sea, and on their distribution, considered as bearing on Geology"—*Rep. Brit. Assoc.*, 1843. "On a Collection of Tertiary Fossils from Malta and Gozo"—*Proceedings of Geol. Soc.*, iv., p. 231. "On the Fossils collected by Lieutenant Spratt in the Fresh-water Tertiary Formation of the Gulf of Smyrna"—*Journ. Geol. Soc.*, i., p. 162. "On the Geology of Lycia"—*Ib.*, ii., p. 8. "On the Fossils collected by Lieutenant Spratt in the Islands of Samos and Eubœa"—*Ib.*, iii., p. 73. "On a Remarkable Phenomenon presented by the Fossils in the Fresh-water Tertiary of the Island of Cos"—*Rep. Brit. Assoc.*, 1845.

draughtsman) were kindly permitted to remain, for the sake of further antiquarian and natural history investigations. Mr Daniel was unfortunately cut off by fever in his prime; but notwithstanding this calamity, the results of a few months' exploration were most satisfactory. No fewer than eighteen ancient cities, the sites of which were unknown to geographers, were examined and determined;* and many valuable facts in geology and zoology ascertained and recorded.

Having successfully accomplished a task, not unattended by difficulty and danger, Mr Forbes was on the point of proceeding to conduct corresponding investigations in the Red Sea, when letters from England announced his (unsought and unthought of) election to the chair of Botany, in King's College, London; an honour not more gratifying than unexpected, as he was not even aware of the lamented death of his predecessor, Professor Don. He was chosen over the heads of several very competent,—indeed, eminent candidates,—without having been a candidate himself. He returned immediately to London, and finding that his professorial duties were confined to the summer season, he sought and obtained the curatorship of the Museum of the Geological Society.

In this superficial sketch we enter not into details. Of Professor Edward Forbes' great excellence as an accurate and philosophical botanist we feel quite assured. One who knew him well, and is highly competent to judge (Dr Joseph Hooker, a kindred spirit), has expressed his wonder that the author of so many and varied geological treatises should have found time to aim at original researches in any other department of science, and should have been so successful in that aim. "This was mainly due to the early age at which he acquired its rudiments; to the efficient practical training in systematic botany and collecting that he received in Edinburgh; to his quick perception of affinities; to his philosophical views of morphology, distribution, structure, functions, and the mutual relations of all these; to his mind being richly stored with the literature of the science; to the wide experience obtained during his travels; and, finally, to that heaven-given power of

---

* See *Travels in Lycia, Milyas, and the Cibyratis,* 2 vols., 1847.

generalization and abstraction which he so eminently possessed."* His introductory address, on assuming the Chair of Botany, was remarkable alike for excellency of expression and originality of thought. It was printed by desire of the Governors and Council. "Those who attended his class will ever remember the charm he threw around the study of vegetable structure, and the delightful hours they spent in his company during the periodical excursions, which he made a point of taking with his pupils, in the neighbourhood of London. Nor were these excursions attended by pupils alone. Many are the distinguished men of science in London who sought the opportunity of availing themselves of his great practical knowledge of every department of natural history."†

One of his most important papers (belonging to an after period) is of a mixed nature, such as he alone could furnish from his own "invincible armoury,"—"On the connection between the Distribution of the existing Fauna and Flora of the British Isles, and the Geological Changes which have affected their area."‡ In this signal work we have opened up to us a wide field of speculative research into almost every department of natural science, while it contains, imbedded in itself, a vast and varied mass of knowledge. It throws a flood of light on some most intricate inquiries regarding the age and relationship of the rocks of Britain.

In 1845 he was offered and accepted the honorable and advantageous appointment of Palæontologist to the Geological Survey of the United Kingdom; and thereafter resigned his situation in the Geological Society, of which at a future period (1853) he was chosen president.§ In connection with this

---

* *Gardeners' Chronicle,* Dec. 2, 1854.

† *Athenæum,* Nov. 25, 1854.

‡ This very remarkable paper is published in the *Memoirs of the Geological Survey of Great Britain,* vol. i., p. 336. Our author's other works, as bearing on Botany, are chiefly these:—"On the Morphology of the Reproductive System of Sertularian Zoophytes, and its analogy with that of Flowering Plants," —*Rep. Brit. Assoc.,* 1844. "On some important analogies between the Animal and Vegetable Kingdoms,"—*Royal Institution,* Feb. 1845. "On the Distribution of Endemic Plants, more especially those of the British Islands, considered with regard to Geological Changes,"—*Rep. Brit. Assoc.,* 1845.

§ His "Anniversary Address" forms a part of the "Proceedings" of the Geological Society for 1854.

department, for the duties of which he was so admirably qualified, we need not do more than name the Palæontological and Geological Map of the British Islands, with explanatory Dissertation, forming part of that now national work, Mr Keith Johnston's *Physical Atlas*, to which Professor Edward Forbes also, and more recently, contributed the map, with letterpress, of the "Distribution of Marine Life." This post of Palæontologist he continued to hold till the period of his death; at least we are not aware that his elevation to the chair of Natural History here—the highest and most influential situation of the kind to be obtained in Britain—led to any change, although some eventual modification might have been found expedient to obviate over-labour on the one hand, or the neglect of scientific business on the other.

His being placed among us here was, indeed, deemed a most fortunate circumstance in relation to the proposed establishment of the so-called Economic or Industrial Museum, forming a branch of, or in some other way intimately connected with, the great zoological and geological collections of the university—themselves about to be, as we and all our community fondly hope, endowed, re-arranged, and opened gratuitously to the public. But where is now the accomplished head and the willing hand, that would have planned so wisely, and so plainly pointed out, the most approved and appropriate courses which we ought to follow,—where the kindly heart and disinterested disposition, which would have smoothed down and overcome the difficulties which cannot but beset the re-construction, on a new, enlarged, untried foundation, of a great scientific Institute about to be unsealed?

But we shall not prolong our mournful meditations on this most sad bereavement, which we really regard as one of the greatest which could have befallen our community. Natural science is necessarily retarded among us for many a day. But let the rising generation bear in mind how much he did with no more assistance than they may still obtain. Let them remember, not only his love of knowledge, and assiduity in its attainment, but more especially his noble and generous temper, ever radiant even in the midst of opposition, like the sun, whose clearness no envious cloud can long encumber, though, when broken and dispersed, it may seem to make his brightness all the more effulgent. Let them think of his simplicity, modesty, freedom from arrogance and affectation, from jealousy and all uncharitableness, and how he ever kept the even tenor of his way, unspoiled by success, unmoved by flattery, fearless in his love of truth, undaunted in his hatred of malevolence and guile. Let not only the young, but also the mature, the middle-aged, the ancient, think of these things.*

" But our idle regrets," says a great and most remarkable observer in the same field, " can neither restore the dead nor benefit the living. Let us rather manifest our regard for the memory of our illustrious brother,—taken so unexpectedly from among us,—by making his disinterested devotion to science our example, and by striving to catch the tone of his frank and generous spirit. And seeing how very much he succeeded in accomplishing within the limits of a life that has, alas ! fallen short by more than thirty years of the old allotted time, let us diligently carry on, in the love of truth, our not unimportant labours, remembering that much may be accomplished in comparatively brief space, if no time be lost, and that to each and all that 'night cometh' at an uncertain hour, under whose dense and unbroken shadow 'no man can work.' " †

---

* So abundant are Professor Edward Forbes' works, that we have not as yet named the most complete and important of them all,—his " Natural History of British Mollusca, and their shells" (in conjunction with Mr Hanley), 4 vols., 1848–53. His latest public efforts were made at the meeting of the British Association, held during last autumn at Liverpool, where he was elected President of the Geological Section. One of his most recent written labours (excepting his engagements with this Journal) was an article in the Quarterly Review, for September 1854, on Sir Roderick Murchison's *Siluria.*

† Address to the Royal Physical Society, by Hugh Miller, Esq.—*Witness,* 29th Nov. 1854.

Introductory Lecture delivered at the opening of the Natural History Class in the University of Edinburgh, on Wednesday, 1st November 1854. By the late EDWARD FORBES, F.R.S., F.G.S., Regius Professor of Natural History.)

[The notes of this Lecture were found among Professor Forbes' Manuscripts, and although probably not intended for publication, they are now printed, in the hope that they will be acceptable to his friends and pupils, and that they wil furnish valuable hints as to the mode of conducting courses of Natural History.

There are few persons who would willingly admit that they know nothing of natural history; and, in one sense, they are right: for, the beauties and curiosities of nature meeting the sight of man at every turn, there can scarcely be a human being, however ignorant and degraded, who has not at some time observed and admired them.

But natural history, properly so-called, is more than this: it is the science of the understanding of natural objects.

When we consider that all objects untransformed by the art of man are *natural*, the vastness of this science in its full extension must be great indeed, for it would embrace all that concerns the earth and its productions, the surrounding air, and extend into the domains of astronomy. But as that which is aimed at by the professorial office is rather the teaching how to study and master a science, through the exposition of its leading facts and laws, than to communicate all that is known about it, to extend the field of our teachings throughout the realms of natural history, would be to prevent the purpose we have in view.

But there are certain great and principal sections of our science which should and will form the substance of our studies here, and which, however various and different they may seem, are in reality intimately and inseparably blended. These are the history of living beings, as they are on our globe and as they were, and the preparation and constitution of the earth's crust for the reception and development of life.

We thus embrace biology, in its more special sense, and geology.

Since the details of one portion of biology, viz., the natural history of plants, are fully taught by one of my colleagues, and since the course of study for which I contend, that which would conduct you to geological knowledge through a preliminary investigation of the classification and characters of living beings, can in the main be effected only through zoology, or the study of the animal part of the creation, it is to the latter division of biology that I shall confine my prelections.

And since, for the understanding of geology (the science to which the latter half of the course will be devoted), an acquaintance with the characters and combinations of minerals is requisite, the sub-science of mineralogy will necessarily form part of our studies.

This, then, will be the order of our work. Commencing with the consideration of those general facts and principles that are common to the several sections of natural history, we shall proceed to the study of existing animals, and through them, arrive at an understanding of extinct forms of life, known only in the fossil state. This department, or paleontology, will, along with mineralogy, form the basis of our enquiry into the structure and geological history of the globe.

Almost all the varied science which we shall have to survey has been eliminated from the facts of nature, within very modern times. Among the ancients, strange as it may seem, little progress appears to have been made in natural history, and the very science, the materials for the study of which lie most abundantly across the pathways of men, was that most neglected, and abandoned to dreamy fable. There are only two authors of antiquity, whose works are preserved, worthy of being cited as original contributors and understanders of science. These are Aristotle and Strabo; the first, unequalled in all times for the grasp of his intellect and the variety of his acquirements, has left in the fragments of his treatise "Πεϱι Σωων," a masterly essay in scientific geology, and a wonderfully accurate statement of well-directed observations. The second, in his geography, the minute accuracy of which

I have admired, when travelling by the guidance of his descriptions, and by them only, through unexplored districts in Western Asia, has in several instances described and commented upon geological phenomena, and started views which for centuries remained unnoticed, because far in advance of their time.

Now it is certainly remarkable that there should be no evidence of any other than these two illustrious philosophers, amongst all the ancients, having made real progress in our science. In all the statements of importance put forth by them, the information is given from their own observation, and no references are made indicative of there having been other men in the field, working in the true spirit of induction, which distinguishes what they themselves did and placed on record. Of other ancient authors whom we are accustomed to quote on account of natural history statements, Dioscorides, although the preserver of much interesting information concerning plants, can scarcely be regarded as more than a herbalist, whilst Arrian and Pliny are in the main compilers, and certainly have no claim to take scientific rank with Aristotle and Strabo.

The building of the great edifice of natural history science was long deferred, although, as we have seen, the corner stones were placed early. During the last 200 years almost everything has been done, and during the latter of these two centuries, the best part of the work. The order of development of the several sections has been in the main empirical. Thus botany advanced first; chiefly through the impulse given to the study by its adoption in schools of medicine, and its connection with the materia medica; zoology passed through many phases, owing much to the systematization of the knowledge of it in his day by the great Linnæus; and more, afterwards, through the wedding of it with comparative anatomy, by John Hunter, Cuvier, and their cotemporaries. Geology, after struggling through the mist of vague speculation, though cheered by occasional and momentary breaks of sunshine, at length, at the beginning of this century, emerged into clear day, and rapidly and steadily advancing, has now taken its just place amongst the foremost and grandest of the sciences.

If we regard the position and condition of the natural history sciences at the present moment, we may consider the age and time most favourable to the successful study of them. But we must not deceive ourselves, and fancy that because natural history is popular, it is therefore generally understood.

Were we to form our opinion from the number of books on all branches of the science, issued almost monthly from the press, in Britain alone, and perused with avidity, we might suppose ourselves a nation of naturalists, and fairly reckon upon finding every tenth educated person we meet versed even in the technicalities of zoology, botany, and geology. Yet is it so? I need scarcely reply in the negative. On the contrary, we are too well aware of the prevailing and wide-spread ignorance of these studies. The fact is this, the books in question are bought and read; the interesting statements they contain excite momentary attention and pleasure; even scientific classifications seem pleasing, because suggestive of well digested order. But the knowledge so gained is word-knowledge only. Now this kind of knowledge can take no root, unless it be accompanied by a knowledge of things and beings. When Oliver Goldsmith, genius as he was, tried his hand at a "History of Animated Nature," and a very delightful book he made of it, he knew so little of the chief subject of his chapters viz., quadrupeds, that he described the cow as casting her horns annually. There is no more dangerous experiment than that of writing about things without a practical acquaintance with them. And there is no information which passes more speedily and thoroughly away from the memory than that of natural history, if it be learned from books only.

The remedy is an easy one. Verify what you read in your book, and hear in your class-room, by observation in the field, and in the museum. Observe for yourselves. Try to decipher the structure, and make out the names of animate and inanimate objects from actual specimens. Even to do this in the most rudimentary fashion is better than to rest content with reading the most lucid descriptions. Many a man can define a vertebrated, an articulate or a radiate animal, without an erroneous expression, and yet be sadly puzzled as to what some unaccustomed specimen placed before him might be. The student who has counted and compared the legs of a fly and a spider, and noticed the resemblance between the segmentation of a centipede and of a layworm, is further advanced in knowledge of the characters of the great articulate group, than he who can repeat whole pages of definition on the subject by heart, and yet would be exceedingly embarrassed were he to be presented with a cockchafer, and called upon to point out those peculiarities in its external organization that distinguish it as an insect.

Many a reader of geological treatises will tell you confidently how the world was made, yet be at his wits' end if requested to name and define specimens of the rocks which he would meet with *in situ* were he to walk from this class-room to the summit of Arthur's seat. I remember some years ago, having a painful interview with a modest and intelligent person, who on account of testimonials and undoubted hard reading, had been appointed to the office of naturalist and geologist in an important foreign expedition. No man could have passed a better oral or written examination upon the sciences required of him, but unluckily all his knowledge of them had been derived from books. He was utterly adrift when asked how he would go to work when he arrived at the scene of his intended labours, and what tools he would use. Still more so when called upon to name a series of specimens of objects with which he would probably have to institute his first comparison. This gentleman, in no spirit of petulance or despair, but simply through an honest sense of his inability to fulfil the task required of him, resigned his mission at once.

Now, I would earnestly urge on every student of this class the necessity of exercising himself frequently in observation of natural objects. My teaching, were it to be as perfect as my utmost ambition would desire, would be of little avail, unless you use your own eyes. Above everything go to the fields, and the seaside. You could not be more favourably situate for out-of-door study than you are here. In a huge metropolis such as London, or even Paris, to make field observations, is to give up entire days to the work. But here the healthy exercise which all of you ought to take, the invigorating stroll around our beautiful neighbourhood, may be made at the same time one of the best scientific lessons. The Queen's Park is a museum of British zoology in itself, and one of the finest natural geological models in the world. The shores of the Frith of Forth are strewn with interesting specimens of marine animals. The very ditches of the meadows, almost within the town, abound in curious freshwater creatures, every one a study in itself. To strolls in the neighbourhood of Edinburgh, whilst a student in the University, I am indebted for much knowledge that has proved to me a never-ceasing pleasure and a benefit in after years.

Do not neglect the museum. It may not be all we could wish, but it is more than enough for supplying the materials of study during the time you can give to it. It has been said of hospitals, that their capacities for instruction are not always in proportion to their vastness, and their number of beds is not of so much consequence as variety and interest of cases. So with museums; it is not mere extent and great accumulations of specimens that render them available for purposes of study, but rather the systematic illustration of the leading types of the several kingdoms of nature. This is the purpose which we shall keep in view in getting our museum here into order, a task that will take some time, but which, nevertheless, is, I trust, advancing. Now, from the types of animal and mineral forms exposed in its rooms and cabinets, you ought to be able to acquire a fair fundamental notion of the science we are met to cultivate. How best to make use of the collection I will explain in another lecture.

The main purpose of your assembling in this class-room is the acquiring a knowledge of the principles of natural history, of the leading facts of zoology and geology, and of the way to go to work in pursuing the practice of these sciences. Within these limits the method of instruction by lectures is well adapted for conveying the requisite information, and forming a basis for more detailed studies in the cabinet and the open air. Moreover, you will thus be guided in the course of study which can only be pursued in your chambers. Study, when desultory and unguided, is rarely beneficial, although better than none at all. When properly and systematically conducted, the study of natural history invigorates the mind, exercises and strengthens the reasoning powers, and educates the observing faculty. For these qualities, it is selected to be a branch of professional education; and those among you who are intended for the noble and self-sacrificing profession of medicine, will never regret having devoted a fair portion of your time to the receiving of zoological, botanical, and geological instruction.

These are days when almost every man, sooner or later in the course of his life, travels, either of necessity, or for purposes of information and amusement. Delightful as it is to explore strange lands, no small part of the pleasure and the benefit of travelling is lost to the man who is ignorant of natural history. The differences between one country and another do not depend wholly upon their inhabitants, their edifices, or their towns. Nature, animate and inanimate, varies in each region of the earth's surface. The differences strike even the uninformed,—but in what manner? Vaguely, dimly, and ignorantly. How often, when we visit foreign countries, do we meet with intelligent travellers, who, perceiving those differences, and unable to comprehend them, lament grievously over their ignorance, and exclaim, "Would that we knew something of natural history!" Often have I heard a like exclamation uttered by the active-minded soldier or sailor, who has longed for occupation in some far-away and lonely station, whence all the sense of loneliness might have been banished, had he been able to observe the wondrous world of living creatures and the construction of the rocky soil around him. Many of you will probably find yourselves under similar circumstances, but, I trust, not under like intellectual difficulties. Learn to observe and to know nature in good time, and you will never be oppressed by listlessness, or wearied through want of objects of interest with which to engage the mind.

Under conditions which to most minds induce hopeless idleness, it is possible for you not only to make yourselves happy, but to gain fame, if that be your ambition, and certainly to contribute, in no small degree, towards the advancement of science. Nay more, under these conditions, you may be in the most favourable position for the perfecting of your own knowledge, and the opening out fresh fountains of discovery.

You will have the advantage over many a naturalist at home; for there is no advantage in our department of science so great as that conferred by travel. The mind becomes warped and narrowed when limited to the contemplation of one set and condition of objects. Observation is exercised, but without the check and gloss of sufficient comparison, and we think we see all, when we are regarding but a fragment. The zoologist and botanist can, it is true, by means of menageries, gardens, and museums, gather together readily the fruits of travel. Still, the natural combinations, so to speak, of living beings, are not fully and fairly seen through such artificial media. The mineralogist can do much in the cabinet and in the laboratory; but there is a mineralogy on a grand scale that must be studied in the open air, and in the recesses of the earth. It is customary to say that minerals are the same everywhere they occur: but this is not strictly true; and the curious and minute differences of constitution, and even of crystallization, which distinguish the minerals of one region from those of another, are indicative of phenomena which have yet to be worked out in the wide geographical fields. The geologist, though he may ground himself thoroughly in his science at home, above all other naturalists, requires to correct and extend his knowledge by wide-spread research and observation; and, when Sir Charles Lyell said that there are three requisites for a geologist, and that these are, "Travel, travel, travel!" he gave that advice which, if it had been the doctrine of the illustrious Werner, would have placed his favourite science in a very different position half a century ago, and freed it at an earlier day from the trammels of local prejudice and partial knowledge.

Now to those who must stay at home—and they are many—the greatest service that can be conferred by him who travels is the communication of correct scientific observations. All of you, then, who look forward to see the wonders of foreign regions, prepare yourselves, in good time, to understand and describe them; and let those to whom the British islands are to be a life residence, learn also, in order that they may understand the new facts that will thus be brought to light.

When urging upon some of my friends the benefit and delight they would derive from natural history studies, I have heard the objection occasionally put forth, that they are incompatible with active professional or business occupations; or, at least, that the carrying them out worthily, and in a spirit of true science, not mere dilletanteism, cannot be effected without an interference with the sterner duties of life.

Plausible as this objection seems, it is not well founded. The proof that it is not so lies in the fact that many of the ablest advancers of the natural history, as well as of other sciences, and, I might add, of literature and philosophy, are men diligently engaged in daily duties of a different kind, and doing their tasks thoroughly and well. The names of many of the most eminent of British men of science are those of fully occupied physicians and successful merchants. Who, for example, have done better service towards the investigation of the zoology of the British Islands than Dr George Johnston of Berwick, and Professor Thomas Bell of London, both carrying out extensive and original researches whilst busily engaged in arduous and never-neglected professional duties? In the last century, Ellis, a busy London merchant, changed the whole face of zoophytology. Only last year died Charles Stokes, a name not popularly known, but very familiar to men of science at home and abroad, similarly occupied with Ellis, who, nevertheless, found time to aid, by his extensive and original knowledge and ever-judicious advice, almost every naturalist of whatever denomination in Europe. At the present moment I could point out several of our very best zoologists and geologists among the most diligent and ablest of British merchants. The law, too, might do much for us, but does not often add to our ranks; yet it is a curious fact, that one of our chief authorities for the anatomy of the invertebrata is a lawyer. The army and navy have more time at their disposal; but it is not among the idle portion of the services that we find the scientific amidst arduous duties; and a naval officer, in command of one of our ships now in the Black Sea, has contrived to acquire and communicate the first satisfactory and scientific information concerning the coal-fields of Asia Minor. Let it not be pleaded, then, that science is to be put aside on account of active professional occupations of any kind. The excuse never comes from the able and willing. It is exactly by the aid of the classes of men who do their professional and business duties best that science has reaped, and is reaping, its most valuable harvests.

To urge upon you the desirability of studying natural history, on account of the material benefits that may result from the pursuit, would be to take a very low ground of persuasion. You do not come here to acquire the art of making fortunes by this kind of learning, but to study it because it is a science worthy of the mind's employment intellectually ennobling in the knowledge it imparts. That which it pleased the Creator to make—the universe and the world on which we live, and the beings that live upon the world with us—these are surely subjects worthy of our deepest study. Every creature, whether existing or extinct, every fragment of rock and constituent mineral, each and all are revelations of Divine wisdom. Now, all which was worthy of God's making is worthy of man's learning, is too plain a truth to need a comment. Well might the old Christian father exclaim, "Creavit angelos in cœlo, vermiculos in terra; non superior in istis, non inferior in illis."

Yet such is the nature of man, that he is constantly harping about things beneath his dignity. The politician, whose business really concerns the fleeting moment, who, whilst he boastfully fancies himself stirring the world—as the fly in the fable stood upon the axle and fancied itself the mover of the wheel—who is useful, because politicians must be as things are constituted, and therefore, and therefore only, respectable—the politician regards the man of science with compassionate concern or supercilious indifference, deeming his pursuits unpractical, because not always useful in the lowest sense. Yet the very politics of the world are changing through the advancement of every form of knowledge, and the development of the character and power of nations depends in no slight degree on the progress of sciences that seem at the moment wholly isolated and theoretical.

Show the man of commerce and the statesman a utilitarian bearing in scientific researches, and all the dignity and vanity of man are forgotten. Show that gold is to be got or to be saved through our work, and the value of our science is at once admitted. Short-sighted diplomatists and sorry economists! The spread of a thirst for pure knowledge is in its results eventually of more benefit, both politically and pecuniary, to the state, than all the immediate "useful applications." A wise people, delighting in intellectual pursuits for their own sake, is a shrewder generation than one lost in money-making and statecraft.

But to get at the mind of a world that values wealth and power as the grand aim of earthly occupation whilst this world lasts, we must occasionally employ its own weapons. It is true that natural history, under its sub-sciences, physics and chemistry, cannot do this very effectively or frequently; but, nevertheless, it has something to say. More especially in its mineral aspects does it bear upon utilitarian interests. In these gold-seeking days, a little knowledge of mineralogy would have prevented the waste of not a little gold. I have seen boxes of yellow mica, imported from California, under the belief that they were filled with the precious metal, and carefully packed prisms of quartz brought home, after being dearly paid for as diamonds, the seller probably having regretted the cheapness at which his necessities compelled him to dispose of them, and the buyer dishonestly chuckling over the goodness of the bargain he had made. On the other hand, I have lately placed, in the cases of the museum, fragments of a mineral that promises to yield a fortune, which lay open, abundantly, to the day, and stood by the roadside unnoticed until it attracted the eye of a scientific observer.

Especially valuable is geological knowledge. Not many years ago, a competent engineer, visiting a district where lime was precious for agricultural purposes, and was procured from a considerable distance inland at much cost, being impressed with the belief, drawn from his geological observations, that there ought to be limestone strata beneath the superficial covering, went to work systematically to test his impression, and ended, to the amazement of the people, by obtaining a lease of the limestone in a district where the natives never heard of its presence. He then made his shafts, and supplied them with the desideratum.

In this case money was *made* by geological knowledge,—oftener it may be prevented being thrown away.

Not a hundred miles from Edinburgh I have seen, since I last lectured in this class-room, costly excavations in progress, the object being a common one—the search after coal in a spot where any geologist would have told the seekers that they might as well throw their money into the sea. In this case a good geologist, who knew the country well, did give timely warning, but in vain. As if to illustrate the absurdity of this wasteful and unscientific experiment, the so-called "practical" men who conducted these operations were actually mining amid vertical strata, sinking their shaft in the dip, and driving their galleries in the strata of the bed; so that, however long they continued their fruitless task, they would be (and possibly at this moment are) constantly working in the same bed in which they commenced.

But I trust that, whilst there shall be no danger of the students of this class making such preposterous blunders, they will always bear in mind the intellectual dignity of the science, and whilst they apply its results to every useful and economical purpose to which they may be adapted, never forget that the grand aim and object is the contemplation and understanding of the greatness and goodness of the Deity, as revealed to us in creation. This purpose constitutes the worthiness of our science, and stamps it with unmistakeable grandeur.

Edinburgh has long been famous as a nursery of naturalists. A large proportion of the most distinguished British zoologists and geologists of our day, and not a few foreign ones, acquired or cherished their taste for the study of nature in this university. The physical advantages of the district have had doubtless much to do in attracting the minds of students to natural history. But these would have been ineffective without the teachings and enthusiasm of my late illustrious predecessor in this chair, who was himself preceded by a less known but able man, Professor Walker, imbued with a like spirit. The eminent men who have gone before me held that the student who aims at being a naturalist, in the proper sense of the word, must combine biological with geological knowledge. For the same view I most strenuously contend. It was the doctrine held and practised by Linnæus, by Cuvier, by Blainville, by Brongniart; and at the present day by such men as Owen, Darwin, and Falconer, all formerly Edinburgh students; by Agassiz, Loven, Phillippi, and Dana. A philosophy of natural history can only spring out of this combination, and can never be evolved from the exclusive study of isolated sections. I hold that the student should begin by taking broad and comprehensive views of the general bearings of the science; and when afterwards, as he must if he is to master it well, he engages in monographic researches, then he will reap the benefit of having laid a foundation of good, sound, general principles.

The day will come when, ere we attempt a complete description and precise definition of any one species of animal or plant, we must first have worked out not only external variations and internal structure, but also the whole history of its distribution in geological time and geographical space.

I am aware that these views are not invariably assented to by the naturalists of the present day, although in favour of them the opinions of the ablest may be cited. I trust to you, gentlemen, for the evidence of their correctness. To the future career of many of you I look forward with hope and confidence. I have had a guarantee of it in the ability and earnestness displayed by many of the students of this class during the past summer. Whatever I can do I will do, and hope you will come to me freely for advice and assistance. We have fine subjects for study; let us go to work earnestly and diligently, and we shall be sure to gain much good scientific knowledge before the winter shall have passed away.

REVIEWS.

Die Conchylien der Nord-Deutschen Tertiär-gebirges. The Fossil Shells of the Tertiary Formations of the North of Germany. By Prof. BEYRICH. Berlin: 1853-4. Parts I.—III.

No one can have directed his attention to a physical map of the North of Europe, excluding, of course, the Scandinavian peninsula, without being struck by the vast extent of the flat, or only very slightly undulating country, which stretches from the south-western frontiers of Belgium through Holland, Oldenburg, Hanover and Prussia, into the very heart of Russia. This relatively low flat region also comprises parts of Silesia and Prussian Poland, with Pomerania and adjacent territories. No inconsiderable portion of this tract consists of unproductive sands, turf bogs, and dreary morasses, occasionally interrupted by districts of diluvial clays, which have been converted into rich and productive meadow lands.

In later times the value of this district has been greatly increased by the discovery of extensive tracts of brown coal, which have been successively worked, and, especially in the neighbourhood of Magdeburg, and of Frankfort-on-the-Oder, supply the inhabitants with a cheap and valuable fuel. The working of these brown coal beds, however, has led to another, and, geologically speaking, still more important discovery. These brown coal beds, derived from the decay of the vegetation of vast lagoons and swamps, form the basis of an interesting series of tertiary deposits, some of which have proved to be unusually rich in the remains of marine mollusca, showing in many districts a remarkable connexion with the well-known tertiaries of Belgium and other countries.

At first, however, they did not meet with all the attention they deserved, and, although the contents of the Septaria clays of Berlin and of Magdeburg, and those of the nodules of Sternberg, have been long known, it is only since the Belgium tertiaries have been worked out by the exertions of Sir Charles Lyell and Professor Dumont, that the attention of the German geologists has been directed to ascertaining their correct position in the tertiary system. Amongst those who have been most active in working out these results is Professor Beyrich of Berlin, the author of the work now under our consideration. It will not, therefore, now be uninteresting to the readers of the Philosophical Journal, to have placed before them a short outline of the work, so far as it is already published, and of the plans and views of the author in carrying out his undertaking.

Professor Beyrich soon recognised the insufficiency of the previously existing catalogues, or lists of names of the Molluscous Fauna of the tertiary beds of North Germany, to enable the geologist to establish a correct comparison between them and the fossils of other countries. They were generally unaccompanied by illustrations. Even the investigations of Philippi respecting the tertiary shells of Cassel, Freden, Zuilkorst, and the neighbourhood of Magdeburg, are not sufficiently comprehensive to enable the geologist to institute exact comparisons between them and the productions of other localities; the progress of the study of the North German tertiaries has consequently been slow. The evil of such an imperfect state of the literature of this branch of science had been acknowledged by the Direction of the Imperial Institute of Geology of Vienna, who immediately prepared the commencement of a separate work on the shells of the tertiary basin of Vienna by Professor Hörnes, in which not only the names but full descriptions and accurate drawings of all known existing species should be given.

Professor Beyrich wishes to do for the North of Germany what Hörnes has undertaken with regard to the Vienna basin.

"It is my intention,'' he observes, "to extend the work to all the tertiary formations which have been discovered, from the frontiers of Belgium and Holland, eastward through North Germany as far as the Oder. All these formations belong undoubtedly to one series of deposits, closely connected with each other, and of which the faunæ are so intimately allied by numerous gradations, that the removal of any single member from the series would destroy the continuity of the whole. In order to have a clear insight into the relative connexions of deposits which occur at such various and distant points, we must bring together for comparison the fossils from the neighbourhood of Dusseldorf, Osnabrück and Bünde, those of Hildesheim and Cassel, those from Lüneburg and the island Sylt, as well as from the neighbourhood of Magdeburg, and from the Markgraviate of Brandenburg. We must also examine the tertiary shells which have been transported into new positions in the diluvial deposits, in order to obtain a perfect view of the molluscous fauna of the tertiary seas of the north of Germany."

The eastern boundary of the country which Professor Beyrich proposes to examine is somewhat artificial, being limited by the extent of our knowledge on the subject. Between the Elbe and the Oder, great progress has been made of late years in the investigations of tertiary geology, while no observations have been made respecting the extension of these fossiliferous tertiary beds beyond the Oder. The author thinks it probable, however, that they nevertheless exist. The geological features of the country form a natural boundary to the south. The Hartz and other mountain districts, which rise more or less abruptly from this northern plain, mark with more or less exactness the limits of the ancient ocean. Alternations of marine and fresh-water deposits are no where met with, nor do any of those combinations of organic forms occur, which are characteristic of brackish waters. This ancient tertiary sea was permanently shut off from those fresh-water basins, which in the interior of Germany formed extensive and perhaps contemporary deposits. The marine tertiary formations, which extend through the countries watered by the Weser as far as Göttingen and Cassel, are a southern prolongation of this North German tertiary deposit, and must be considered as separated from the north-eastern prolongations of the Mayence basin, which is characterized by its peculiar composition, and the abnormal development of its fauna.

We cannot state thus generally the views of Professor Beyrich without adding one or two remarks, modifying, in some degree, the universality of the expressions. When Professor Beyrich states that there is no alternation of marine and fresh-water deposits, he surely cannot have overlooked the fact that these marine formations almost everywhere overlie the brown coal, and that although no animal remains have been found in this brown coal, it must be looked upon as a fresh-water deposit formed in vast lagoons or swamps probably at no great elevation above the then level of the ocean, and derived from the decay of fresh-water vegetable matter. In the next place it appears to us that in the present state of our knowledge, it is somewhat arbitrary to attempt on the one hand to connect the tertiary beds of Cassel, Bünde, Gottingen, &c., with those of North Germany, from which they are separated by mountain ranges of considerable elevation, and on the other to cut off these same Cassel tertiaries from the North Eastern prolongations of the Mayence Basin with which the physical, and, to a certain extent also, the mineralogical connection appears to have been both natural and continuous.

The author then proceeds to show the importance of instituting a comparison between the tertiaries of Belgium and those of North Germany, observing that, although the time is not yet come for the complete development of this parallelism, there are certain established points of connection which must not be lost sight of.

After explaining Dumont's five systems (Landenien, Ypresien, Panisilien, Bruxellien, and Laekenien), which, taken together, are the equivalents of the Paris Eocene formations up to the sand of Beauchamp, and of those of England up to the Barton clay, he observes :—" Hitherto we know of no fossils from any part of the North of Germany which positively prove the existence of tertiary deposits of so great an age. The oldest North German tertiary Fauna, viz., that of what I have called the Magdeburg Sands agrees rather with that of Lethen in Belgium, which belongs to the lower portion of the Tongrian (Systeme Tongrien), and immediately overlies the Systeme Laekenien, the uppermost of the five Systems of Dumont just alluded to. Moreover, the occurrence of this Fauna is as yet confined in North Germany to the country west of the Elbe between Magdeburg, Calbe, and Egeln."

The next fossiliferous bed in ascending order which occurs in Northern Germany is the Septaria Clay of Berlin, which, with its characteristic fossils, has hitherto been found near Stettin, Freienwalde, Bukow, Hermsdorf, and Lübars near Berlin, Burg, Hohenwarthe on the Elbe below Magdeburg, and Görzig near Köthen. The same clay occurs in an isolated position in the Lünebürger Heath at Walle, near Celle, but it is not again met with in a westerly direction nearer than Belgium, where the clay of Boom Baesele and other places south of Antwerp is perfectly identical.

Professor Beyrich refers the Fauna of the Sternberger beds to the same Belgian System (Systeme Rupelien). They contain the characteristic shells of the Septaria clay, with others which are not found in the older beds. It also occurs in the neighbourhood of Stettin. The author is still uncertain whether any beds occur in North Germany corresponding with the deposits of Kleyn-Spawen, placed by Dumont between the Rupelmonde Clay and that of Lethen, and which are referred partially to the Rupelmonde, and partly to the Tongrian System. This is important because these are the beds which, as De Koninek first suggested, have the greatest analogy with those of the Mayence basin.

All the tertiary deposits of the lower Elbe belong to a more recent period, as well as those of other more northern localities near Lüneburg, Hamburg, and Holstein, and those of the island Sylt and Schleswig. Of the same age are those observed by F. Roemer on the frontier of Holland, and by Acfeld and Düsseldorf. They must not, however, be placed higher than the deposits of Bordeaux, the Touraine, Turin, and Vienna. Deposits of the age of the clay of England and of Antwerp are altogether wanting in North Germany. The youngest tertiary deposits of North Germany belong to the Bolderberg System, which is placed by Dumont and Lyell as parallel with the typical Miocene formations of France and other countries, and of which, although inferior to that of the Vienna basin, it is a better representation than the Belgian deposit.

After thus describing the physical characters of the North German tertiary deposits, the author proceeds to discuss the question as to where the boundary line is to be drawn between the Eocene and Miocene formations in Belgium; and after fairly stating the views of Dumont, Lyell, and d'Orbigny, he appeals to the evidence of North Germany, from which it appears that, in so far as the lowest beds of the Tongrian System appear as the base of the whole marine tertiary formation of the North of Germany, to the total exclusion of all older formations, this is an important geological support to the view of Sir C. Lyell, that a stronger line of separation is to be drawn between the Laeken and Tongrian systems rather than between the Tongrian and the Rupelmonde systems; but he does not agree with Lyell in giving to these united systems the name of Upper Eocene rather than Lower Miocene—he rather adopts the views of the French Palæontologist in considering them the forerunners of the Miocene formation, and is therefore prepared to call them Lower Miocene.

After alluding to the different suggestions of Dumont and others for various subdivisions of the tertiary formation, he observes, (while at the same time refusing to be bound to the mere artificial rule of percentages), that the terms Eocene, Miocene, and Pliocene, should be maintained as representing periods of time, the centres of which are well known to us, but whose beginnings and ends run into each other; in the same way as, the more our knowledge is extended, we find to be more and more the case in all investigations respecting geological periods.

In conclusion, the author adds a few words respecting the arrangement and the form in which he proposes to give his description of the north German tertiary shells. The Univalves precede the Bivalves; and adopting the plan of Hörnes's work on the Vienna Basin, he commences with the Gastropods. This has the advantage of establishing a more easy system of comparison between the two formations; and with the same highly laudable view he has determined to adopt the same order of genera. This is the more praiseworthy, as he admits that in some instances a more satisfactory arrangement might have been adopted. Such a sacrifice of personal views is the more to be admired in proportion as it is rare; and the advantage to students of the two systems cannot be questioned. He has wisely determined not to overload his work with too much description, or the useless repetition of synonyms already published in so many other standard works.

To give some idea of the extent of the work, we add a list of the genera already published, with the number of species belonging to each genus:—Voluta, 10 species; Mitra, 11; Columbella, 3; Terebra, 6; Buccinum, 13; Purpura, 2; Cassis, 7; Cassidaria, 3; Rostellaria, 2; Aporrhais, 2. Total,—60 species on 15 plates.

We cannot conclude these remarks without thus publicly awarding our thanks to Prof. Beyrich for having undertaken this work. It is evident that we can have no correct idea of the real nature of the successive *facies* of the Molluscan fauna of the Northern Ocean without it. It will indirectly tend to give us more correct views of our own interesting tertiary formations, and thus lead to a truer knowledge of the various gradations through which the creation of tertiary forms have proceeded from the close of the cretaceous epoch down to its most recent deposits; and while, on the one hand, we urge Prof. Beyrich to advance in his great work as rapidly as circumstances will allow him, we must also express a hope that he will meet with such encouragement from British Palæontologists, as will prove to him that his labours are fully appreciated in the country of a Lyell and a Forbes.

Memoirs of the Life and Scientific Researches of John Dalton, Hon. D.C.L., Oxford, &c. By WILLIAM CHARLES HENRY, M.D., F.R.S. Printed for the Cavendish Society, 1854.

We have now the satisfaction of welcoming a work on Dalton, which leaves us nothing to desire, so far as regards his personal history or his scientific labours. His history was eventless, his nature unimpassioned, his intellect clear and self-reliant, and his perseverance inexhaustible. By many and slow steps he won his way to reputation, and what to so modest a philosopher seemed wealth, was added to fame.

Born in 1766 in Cumberland, the son of a yeoman, whose small copyhold afforded no patrimony for a younger brother, Dalton shared in the labours of his father's farm during the summer months, and in addition commenced at the precocious age of twelve, to teach a school in his native village. When fifteen years old, he removed to Kendal, and along with his elder brother Jonathan, conducted a seminary for children of members of the Society of Friends, among whom the Daltons had been numbered for three generations.

In the humble office of schoolmaster, he continued at Kendal for eight years, devoting his leisure to the study of mathematics, natural philosophy, chemistry, and the languages, in the prosecution of which he was encouraged and assisted by Mr Gough, a blind gentleman of remarkable acquirements, who set him the example of keeping a meteorological register. For this the continually changing aspects of such a district as that around Kendal, with its hills and dales, and sheets of water, presented peculiar facilities, and Dalton soon became an enthusiastic meteorologist, and continued one to the last. Round meteorology, indeed, all his researches naturally grouped themselves, and it was originally to solve important problems in the science, which he had more or less cultivated for twenty years among his native hills, that he entered upon those enquiries into the laws of Heat, the Constitution of Gases, and the Composition of Chemical Compounds, which afterwards made him so famous.

The first of his scientific publications, "Meteorological Observations and Essays," appeared in 1793, soon after his removal to Manchester, to enter on the office of Tutor in Mathematics and Natural Philosophy in a Dissenting College in that town. He resigned this appointment at the end of six years, but continued to reside in Manchester to the close of his days. It is not our intention here to trace the events of his personal history: it will suffice, therefore, to state that the reputation he acquired by his Meteorological Essays, was greatly increased by the publication in the Manchester Philosophical Memoirs, from 1799 onwards to 1801, of Essays on Evaporation; on the conduction of heat by liquids; on the constitution of mixed gases; on the force of steam or vapour from water, and other liquids; on evaporation; and on the expansion of gases by heat.

These remarkable papers attracted the notice of the scientific world and led to Dalton's invitation to lecture at the Royal Institution, London, in 1804, where Davy was then delighting auditors of all ranks and professions by his chemical prelections. In the short course of lectures Dalton delivered at this time, he announced the results of researches, which were not published till 1805. These embraced an experimental enquiry into the elastic fluids of the atmosphere; an investigation into the diffusion of gases; and a Memoir on the absorption of gases by water. It was this last paper, read to the Manchester Society in 1803, but not published till 1805, which contained what its author called a "Table of the relative weights of the ultimate particles of gaseous and other bodies;" or what we should now name a Table of Atomic Weights. It was the first such Table, and was destined more than any of his publications to make its author memorable.

He was led to construct such a Table originally from the desire to solve a problem important to meteorology: "why is one gas more soluble in water than another?" He thought the different solubilities of gases might prove to depend on the unlike size of those ultimate particles, which he afterwards named atoms, and regarded as so essentially indivisible that he enforced on his pupil, Mr Ransome, that a law of Multiple Proportion could not fail to exist, in these *naïve*, but most expressive words—"Thou knows IT MUST BE so, for no man can *split an atom!*" (*Life*, p. 222.)

From this time forward, chemistry much more largely occupied his time than before, and fully alive to the novelty and importance of his views on atomics, he proceeded to embody them in a work which his modesty and simplicity of character did not prevent him from naming a "*New System of Chemical Philosophy;*" a title which the scientific world cordially and admiringly received and ratified. The first part of vol. I. of the *New System* was not published till 1808, and the second not till 1810. The second volume did not appear till 1827. It contained in an Appendix, what its author styled a "Reformed" Table of Atomic Weights, in which oxygen figures as 7: nitrogen as 5 + or 10?; carbon as 5·4; sulphur 13 or 14; and phosphorus as 9; hydrogen being regarded as unity. It is not a little remarkable that the author of the atomic theory was wrong, and far wrong, in every one of his atomic weights. He would accept none of the corrections of other chemists, and priding himself on his practicality defended all his numbers, which are now universally discarded; but it was this stubborn self-reliance which enabled him to transcend the imperfection of his self-supplied data, and by the power of his genius to announce laws, which, paradoxical though it may appear, he established as true, although every example of their truth he offered was false.

Dr Henry's work enables us to dispose conclusively of the much-vexed question how far Dalton was anticipated by others in his announcement of those laws of combining proportion by weight, which obtain in chemistry. His biographer's revelations strikingly show how difficult a task it ever is to write history faithfully, and how little even the most able and friendly contemporaries of a man can often be trusted in their estimate of his doings. Every chemist was aware that Dalton had been anticipated in the discovery of the law of Reciprocal Proportion, by Richter (following out the views of Bergman and Wenzel), not to mention the law of Definite or Constant Proportion, which he did not claim as his own; and that Higgins had preceded him in regarding chemical combination as occurring between the ultimate particles of bodies. At the same time, it was matter of almost total uncertainty how far Dalton, who read exceedingly few books, was familiar with those earlier researches; but the general impression, advocated in his own behalf by Higgins, and so far favoured by Davy, was, that Dalton had some acquaintance with Higgins's views, but none, as Dr T. Thomson specially asserted, with those of Richter.

It now appears that Dalton was ignorant altogether of the existence of Higgins or his writings, till many years after he published his views on atomics; and Dr Henry shows very distinctly that though Higgins did not hesitate to hint at plagiarism, his doctrines, however ingenious, are inconsistent with each other, and are not based on such considerations as led Dalton to his conclusions.

On the other hand, his biographer gathered from the lips of the chemist himself, that he had profoundly studied Richter's tables of combining proportions before he published his Atomic Theory; but it does not less clearly appear, that before he was familiar with the views of the German chemists, he had not only realized very clearly the existence of what are now termed the laws of Constant and of Reciprocal Proportion, but had discovered the law of Multiple Proportion, which no one had even suspected to exist, before he announced it; and had in effect announced the equally important law of Compound Proportion, the honour of proclaiming which no one disputes with him.

Dr Henry also shows more fully than had been shown before, that with an almost inexplicable perversity, Dalton insisted on disbelieving in those beautiful laws of Combination by Measure, which Gay-Lussac proved to obtain in the case of gases, and entitled the Theory of Gaseous Volumes, although it was the counterpart of his own theory of Combination by Weight, and, as every one now sees, confirmed and extended it.

Dalton died a believer in the existence of atoms which "no man can split." His biographer has marshalled with great fulness and clearness all the arguments deducible from recent chemical discoveries and speculations, in support of the existence of indivisible ultimate particles, or true atoms; but he impartially acknowledges that they cannot be *demonstrated* to exist, and contents himself with urging the probability of their existence. The important and much disputed question here raised, we shall not discuss on this occasion, but all to whom it is interesting will find new and valuable materials for its settlement in Dr Henry's work.

It remains to add, that on the *personelle* of Dalton, of which we have said nothing, ample and very interesting particulars are furnished; and that the volume is enriched by contributions from many distinguished men of science. The Cavendish Society has done a signal service in publishing a work so well written and so valuable.

The Principles of Harmony and Contrast of Colours, and their Applications to the Arts. By M. E. Chevreul, Membre de l'Institut de France, &c., &c. Translated from the French by Charles Martel. London: Longman & Co. 1854.

Chevreul is a remarkable example of distinction won in departments of enquiry so different, that posterity is likely to halve or double him, and insist on the existence of at least of two Messieurs Chevreul, the one famous amongst chemists, as the discoverer of the true nature of Fatty bodies; the other, a high authority among Natural Philosophers and Artists, as a discoverer of new relations among colours. There is, however, but one Chevreul, and his work on colour, which sprang out of his labours as chemist to the Gobelins tapestry dye-works, stands in natural and pleasing association with his purely chemical investigations.

His views upon colour have been so long and so highly appreciated on the Continent, and especially in France, that our foreign brethren have naturally wondered that we have been so tardy in acknowledging their value, especially in their application to the practical chromatic arts. Our natural philosophers did not overlook their importance, as our university libraries can testify; and in 1848, the Cavendish Society published an admirable abstract of Chevreul's views, of the existence of which the translator of the work before us appears to be quite ignorant. It was not, however, till the Great Exhibition in 1851, that the conspicuous superiority of the French coloured designs drove our workmen to discover the cause of their own inferiority, and the continual reference to Chevreul as one of the great authors of the skilful use of colours by the French dyers, weavers, and other workers in the chromatic arts, turned the attention of practical men in this country to his book. The volume before us is the fruit of the interest thus awakened in the author's researches, and we welcome its appearance in an English form.

Large as the work is, it is the demonstration of a single fertile principle, which its author calls the "Law of the Simultaneous Contrast of Colours." The purport of this law, is to point out the singular fact, that when two coloured objects, such for example as a red and a green ribbon, are placed side by side, or so near each other as to be seen together, the quality and intensity of their respective colours do not appear the same as when each is looked at separately. Thus, the same red ribbon will have a different tint if seen side by side with a green, with a yellow, and with a blue ribbon, and these colours will in their turn be modified to the eye, by their juxtaposition with red. This is the *Simultaneous Contrast of Colour.* If, again, two shades or tints of the *same* colour be placed together,—for example, a light red, and a dark red, the latter will appear darker, and the former lighter, than either does when seen alone. This is the *Simultaneous Contrast of Tone;* the word "tone," being used by Chevreul as synonymous with intensity of tint or shade, not as referring to any real or supposed analogy between colour and sound.

So far as tone is concerned, the rule is sufficiently noticed above. As for contrast of colour, it occurs according to the principle that every colour adds its complementary to the colour it is placed near or beside. Thus, red causes other colours near it to appear as if its complementary green were added to them. Green tints them with red. Blue adds to other colours orange. Yellow adds to them purple. The appearance of any coloured body beside another coloured body, is thus different from what it is when seen alone or on a white ground, and the difference is such as would be produced by adding to the isolated colour so much of the complement of the colour which by its proximity, modifies it.

It had long been known, as Chevreul amply acknowledges, that when the eye is *fatigued* by looking at one colour it sees its complementary; but it was reserved for him to show that fatigue is not essential to the development of the phenomenon, or rather that there are two phenomena which have been confounded together,—the one, long observed, where the eye gazing long on one colour, sees thereafter on white surfaces its complementary; the other that discovered by Chevreul, where the colour and its complement are seen side by side. The former he names the *Successive* contrast; his own discovery the *Simultaneous* Contrast of Colours; and he points out very clearly that the phenomena may intermingle so as to give rise to what he calls *Mixed* contrast of colours.

The application of those observations to the practice of the chromatic arts is carried out by Chevreul in the most elaborate and interesting way. With the utmost patience, conscientiousness, and sagacity, he illustrates the light which his discoveries throw on the details of painting, glass-staining, tapestry-weaving, carpet-making, the selection of furniture, the arrangement of flowers in gardens, the provision of uniforms for soldiers, the choice of linings for ladies' bonnets, and much else.

Those things lie beyond our sphere, but we could wish that some of our writers who publish on the Harmony of Colours in organised beings would study Chevreul. They might find that they had been long anticipated, and even surpassed. Much, for example, has been said regarding the occurrence of complementary colours in flowers and birds, as if the discovery were something new. It is not only old, but those who read the book will find that an explanation (as we venture at least to suggest) of the pleasure with which the complementary colours, such as red and green associated in plants and in birds, is to be found in the fact pointed out by Chevreul, that when complementary colours are placed together, each exalts the other, so that red makes green greener, and green makes red redder, than either would appear alone. The eye is gratified with the full colour in these cases, not in virtue of some vague recognition of complementaries, but because by no other arrangement can two colours be made to show so fully and richly.

We cannot forbear stating that justice is not done to Chevreul in the present translation. It is awkward, inelegant, often barbarous in style, and sometimes quite unintelligible. Uncoloured diagrams, also, are employed in illustrating the work, but they are most inadequate; and the plea for omitting colours, that the reader can make such for himself is untenable; for a reader skilful enough to do that need not study Chevreul.

CORRESPONDENCE.

Letter from Mr M'ANDREW *to* Dr BALFOUR, *relative to a Communication from the late Professor E. Forbes.*

A few notes for a paper "On some points concerning the Natural History of the Azores, by the late Professor E. Forbes," have been placed in my hands for elucidation. They are the result of information furnished by me, and as my lamented friend pointed out to me the bearing which such information had upon his Theory concerning the Origin of the Fauna and Flora of the British Islands, I am enabled to furnish the following statement, which may not be without interest, as recording and explaining certain opinions of the eminent naturalist, who has just been taken from among us.

Professor E. Forbes has stated, that when in 1846 he published in the 1st volume of the Memoirs of the Geological Survey of Great Britain, his essay "On the Connection between the Distribution of the existing Fauna and Flora of the British Isles, and the Geological Changes which have affected their area, especially during the epoch of the Northern Drift," his theory, that previous to or during the glacial period, the Continent of Europe had extended as far west as the Azores, was inferred from geological and botanical phenomena, and that at that time there were no data accessible for testing his opinion, by reference to *animal* life. He says, that if his views were correct, then the terrestrial and marine moluscs of the Azores should be neither peculiar nor American, but Lusitanian types, and species identical with Portuguese molluscs, or those inhabiting the coasts and shores of Madeira and the Canaries. "This question," he continues, "may now be said in a great measure to be answered;" and he refers to the accompanying list of 52 species of marine, and 20 species of land mollusca* collected in the Azores by my son, James J. M'Andrew, during the last winter. Of these, he states, that all the marine, except two or three critical forms, are Lusitanian, or in a few instances, Canarian species; and that of the land shells, only three are undescribed types, the remainder being common to the Lusitanian or Atlantic Island fauna. These facts he considers as fully supporting his theory.

In the notes before me, Professor Forbes also calls attention to

---

* I have ventured to make a few corrections in the lists, omitting Mitra nigra, which is identical with M. fusca, and adding Helix lactea, H. crystallina, and H. barbula (Moulet.) The latter received by Professor E. Forbes from Fayal, is, of all, the most peculiarly Lusitanian, having, to the best of my knowledge, only been obtained previously in Portugal and the adjoining Province of Gallicia in Spain.

the curious fact, that whilst both the land and marine shells are chiefly of European species, the littoral portion of the latter are mostly of African type, common to West Africa, as well as to the Canary and Madeira Islands. He has not given any interpretation of this significant record of changes which the earth must have undergone since the introduction of the existing fauna, and which may possibly be deserving of the attention of geologists.

The preceding facts are all that are referred to in the notes before me, and I only hope that I have succeeded in stating them intelligibly.

ROBT. M'ANDREW.

LIVERPOOL, 19*th Dec.* 1854.

List of Shells obtained from the Azores.

Marine Mollusca.

Chiton fascicularis—(Celtic and Lusitanian.)
Patella vulgata—(Celtic and Lusitanian.)
Acmæa Gussoni—(Mediterranean and Canaries.)
,, parva—(Celtic.)
Emarginula (pink)—(Madeira.)
Fissurella—(Mediterranean.)
Haliotis (tuberculatus, var.?)—(Lusitanian.)
Trochus Langieri—(Portugal and Mediterranean.)
,, striatus, var.—(Portugal and Mediterranean.)
,, (monodonta) Berthelotti—Canaries and Madeira.)
Turbo rugosus—(Lusitanian.)
Phasianella pullus?—(Lusitanian.)
Fossarus Adamsoni (littoral)—(Africa and Canaries.)
Littorina striata (littoral)—(Africa and Canaries.)
Rissoa cingillus—(Celtic.)
,, crenatus—(Mediterranean and Canaries.)
,, clathrus—(Celtic and Lusitanian.)
,, cimex—(Lusitanian.)
,, new species?
,, new species?
Cerithium adversum—(Celtic and Lusitanian.)
,, reticulatum—(Celtic and Lusitanian.)
Scalaria clathratula—(Celtic and Lusitanian.)
Eulima Boscii?—(Canaries and Madeira.)
,, distorta—(Canaries, Celtic, and Lusitanian.)
Natica intricata?—(Lusitanian and Canaries.)
Chemnitzia elegantissima—(Celtic and Lusitanian.)
Mitra fusca (littoral)—(Africa, Canaries, &c.)
,, zebrina (littoral)—(Africa, Canaries, &c.)
Mangelia septangularis?—(Celtic.)
" (or Columbella) (Peculiar.)
Nassu incrassata—(Celtic and Lusitanian.)
Columbella rustica—(Lusitanian.)
,, cribraria??—(Canaries.)
Murex corallinus—(Lusitanian.)
Purpura hæmastoma—(Lusitanian and Canaries.)
Triton nodosum (dwarf var.)—(Lusitanian and Canaries.)
,, tuberosum—(Canaries.)
,, scrobiculatum—(Mediterranean.)
Cypræa pulex—(Mediterranean.)
Pedipes (littoral)—Africa and Canaries.)
Conovulus albus—(Celtic.)
Ianthina fragilis } floaters.
,, exigua }
Spirula Peronii—(Portugal, Canaries, &c.)
Tapes
Cardium papyraceum—(Mediterranean and Canaries.)
Cardita calyculata—(Mediterranean and Canaries.)
Ervilia castanea—(Portugal, Canaries, and Mediterranean.)
Cytheria Chione—(Lusitanian.)
Pecten pusio—(Celtic and Lusitanian.)
Lima hians—(Celtic and Lusitanian.)

Land Mollusca.

Testacellus Maugei ? from St Mary's.
Vitrina Lamarkii ? from St Michael's.
Helix aspersa, from St Michael's.
  „ lactea, from St Mary's.
  „ lenticulata, from St Michael's and St Mary's.
  „ rotundata, from St Michael's and St Mary's.
  „ crystallina, from St Michael's.
  „ cellaria or lurida, from St Michael's.
  „ rubescens, var., from St Michael's.
  „ new species, like arbustum, from St Michael's.
Helix, new species ? from St Michael's and St Mary's.
  „ new species ? from St Michael's and St Mary's,
Bulimus decollatus, from St Mary's.
  „ species allied to B. pupa ? from St Michael's and St Mary's.
  „ new species ? from St Michael's.
  „ ventrosus, from St Mary's.
Zua lubrica, from St Michael's.
Balea fragilis, var., from St Michael's.
Pupa compostoma ? from St Michael's.
Limax cinereus, from St Michael's.

Selwyn on Australian Geology.—The following notice of various points connected with the geology of the colony of Victoria, is extracted from a letter (19th May 1854,) from Mr Alfred Selwyn to Professor Ramsay:—High results would accrue to geological science were more of our colonies examined in the able and systematic manner followed by Mr Selwyn, who has now been for about two years in charge of the geological survey of the colony, after having been for about six years actively and ably engaged in the geological survey of Great Britain. The notice possesses a melancholy interest from the mention of that gifted man, whose untimely death will long be felt and deplored by geologists in every quarter of the world.

" For the last three months I have been at work between Melbourne, Port-Philip Head, and Western Port Bay. I have found and collected a considerable number of tertiary fossils, mostly in a stratum of blue stiff clay, containing bands and nodules of hard grey limestone, with veins containing sulphur and fine crystals of selenite, the whole very like the London clay, or Barton Cliff, Hampshire. Among the fossils are terebratula, and a few other bivalves, turitella, vermetus, patella, nautilus, murex, buccinum, &c. I shall send a quantity home to Forbes by first opportunity. I have found them in only one place, on the east side of Port-Philip Bay.

" I think I mentioned in my last, that I had found fossils, apparently Lower Silurian, in the auriferous rocks at Mount Ivor, fifteen or twenty miles east of Mount Alexander.

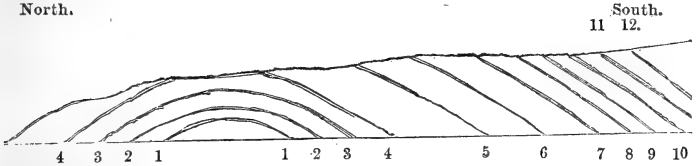
" Another matter of great interest, and one I mentioned in a letter to Jukes some twelve months since, is now proved beyond a doubt, viz., the extension of the auriferous drifts under the great lava plains of the rivers Loddow, Campaspie, &c. At the very place where I first saw some evidence of such being the case, they are now sinking through the lava down into the auriferous drift. I have also lately seen several small grains of native tin mixed with the gold from the Ovens and Ballarat. This is, I believe, uncommon.

"To the eastward of West Port Bay the country has never been explored. I intended to have begun an examination of it this autumn, but the wet weather having set in a month earlier than usual, has obliged me to defer it till next summer, it being a very difficult country to penetrate. There are no roads, and many steep ranges covered with dense scrubs and thickly timbered. I know it to be for the most part coal measures, and in this district it is, if anywhere, that workable beds of coal, are likely to be discovered. From what I have seen of the coal measure beds, they seem to consist chiefly of thick bedded soft sandstones, green, brown, and yellow, of various shades, and I think they are quite unconformable on the older (Silurian or Cambrian) palæozoic auriferous rocks. Of this, however, I have no certain proof at present.

"The traps and basalt of the Western Port district, are evidently of much older date than the great lava plains in the vicinity of the diggings, which are the products of recent volcanoes, while the former are, I should say, igneous, but not strictly volcanic. All the districts occupied by the older igneous rocks are hilly, scrubby, and densely timbered, while those occupied by the volcanic rocks are open grassy plains, almost destitute of timber, with a few scattered conical hills, apparently, for the most part, craters, or points of eruption. There are, I find, traditions amongst the aborigines of some of these hills having been seen on fire by their ancestors, which does not seem improbable. In one or two I have visited, the craters are distinctly visible with a small gap broken down on one side of the wall."

Spratt on the Occurrence of Coal in Turkey.—In the present juncture of affairs, the following extract from a Letter from that able geologist, Commander SPRATT of the Spitfire, to Professor FORBES, is of much interest, showing the possible supplies of coal that may be obtained by Government for our steamers in the East.

"I am truly glad to hear that the Kosloo coal proves to be of the true carboniferous age by the fossils, as the governments here are said to have some intention to work the mines by English miners, and by their knowing that the district is really so valuable and promising, they may be induced to secure to themselves a deeper share and interest in the working of the district; for the coal may be found in almost every valley betweeen Erakle and Amastris, at from one-half to seven or eight miles, and at various elevations from 50 to nearly 1000 feet, and there are many valleys which open into the sea on this line of coast. A fine speculation is open here, the coal being found above the surface of the sea at all angles on the sides of the valleys. It has been much disturbed, and seems to lie in undulating basins like the Belgian, having been subjected to a great lateral pressure during the disturbance and uplifting of the strata.



Rough Section (two miles) of the ridge on the east side of the Kosloo valley with its numerous coal seams. Many faults displace the seams varying from a few inches to several feet.

No. 1 Coal-seam.
„ 2 Coal 18 feet.
„ 3 „ 3 feet bad, excepting 10 inches.
„ 4 „ 5 feet bad, soft, dip 30 degrees on north side; 4 feet, 6 inches, soft, on south side.
„ 5 „ 4 feet, 10 inches, roof of conglomerate, containing quartz pebbles, with 6 inches of shale between it and the coal.
„ 6 „ 5 feet soft.
, 7 „ 4 feet 10 inches very good—best.
„ 8 „ 5 feet.
„ 9 „ 5 feet.
„ 10 „ Coal-seam.
„ 11 „ 9 feet.
„ 12 „ 5 feet, dip south-east, 32 degrees.

"I give you here a list of the valleys with coal, some of which I have crossed on my route from Kosloo to Erakle, or Erayle as it is pronounced. The details were given me by my intelligent friend Mr Barkley, the civil-engineer working the mines for the Turks, to whom the development of these resources is mainly due. I procured from him one or two specimens of the fossils when there, which he had in his house, and also worked at the pit's mouth the next day, and thus found what was sent you.

Localities with Coal.

Amont Keni.—Nine miles from Erakle, one mile from the sea—a seam of very good coal, cropping out on the side of the valley.

Ali Jaza.—Two seams worked by Croat squatters, five and eight feet each, dipping 70° to north-east, and one and a half mile from the sea.

Tchonsh Jaza.—Two seams, both coal.

Ooloosoo.—Twenty miles from Erakle, has a thermal spring 70°, and a good seam of coal just discovered.

Okoosnu.—Thirty miles from Erakle. Has several seams on the summit of the mountain, two and a half miles from the sea. The coal lies of various inclinations, and is of good quality.

Zunzeldek.—Three miles east of Kosloo; has seven or eight seams; very similar to the Kosloo, and varying in their size and inclinations.

Baluk and Uzulmas.—Several seams in these valleys, one having a seam of good coal, twelve feet thick, about two miles from the sea; all variously inclined.

"The coal is interstratified with shales, sandstones, and conglomerates of quartzose pebbles, with occasional bands of clay. The whole overlying a mass of gray limestone, and apparently unconformable, but upon this I could not satisfy myself fully; indeed it would require a series of observations during many days to describe the district thoroughly, and I have not time now to refer to my journal and note book, to refresh my memory fully upon it. The coal measures pass upwards into a friable reddish shale, near Erakle, where they have been baked by volcanic outpourings, viz., streams of serpentine, greenstone, and trachyte pebbles; and, at Erakle, we have these upper shales on the coast, with some fossil oysters in them of a very small size."

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Monday, Dec. 4, 1854. Right Rev. Bishop Terrot, V.P., in the Chair.

Farther Experiments and Remarks on the Measurement of Heights by the Boiling Point of Water. By Professor J. D. Forbes, —This paper is in continuation of one printed in vol. xv. of the Royal Society's Transactions, and in a previous number of this Journal. The object of it is to test the correctness of the method of observation, and of calculating the results, then proposed, and to compare both with those of more recent authors, particularly of M. Regnault of Paris, and of Dr Joseph D. Hooker.

The author finds the results of his subsequent observations in 1846 in the Alps, up to heights considerably above 10,000 feet, to agree well with those previously published, made in 1842. They combine in shewing a sensibly uniform fall of the boiling point at the rate of 1° for 543 feet of ascent (in a standard atmosphere at 32° of temperature), which differs only 6 feet (in defect) from his previous determination. The average deviation of the individual results from the formula is only $\frac{1}{12}$ of a degree (without regard to sign).

Barometer.	Boiling Point.	Difference from My formula.	Difference from Regnault's Formula.
Inches.	Fahr.		
20 77	194°·28	+ 0°·22	+ 0°·32
20·79	194·33	– 0·08	+ 0·01
22·40	197·94	– 0·04	+ 0·12
22·67	198·51	– 0·08	+ 0·06
23·15	199·52	– 0·07	+ 0·06
23·35	199·94	+ 0·01	+ 0·15
23·89	201·04	– 0·11	+ 0·03
23·99	201·24	– 0·09	+ 0·08
24·02	201·31	+ 0·04	– 0·20
24·105	201·47	– 0·17	+ 0·03
25·14	203·51	+ 0·04	+ 0·19
28·49	209·54	– 0·07	– 0·06

The agreement with M. Regnault's table is also extremely close; and considering the ordinary limits of error of such observations, the writer considers it nearly indifferent for elevations under 13,000 feet which method of calculation be used.

The consistency of the results shews that the method of observation (which differs in some respects from that commonly used) and the graduation of the thermometers were satisfactory.

On carefully examining Dr Joseph Hooker's detailed results, (obligingly communicated by him), which that naturalist considered to be incompatible with Professor Forbes's formula, it is shewn that the inconsistencies of observation are so considerable, that it is difficult to give a decided preference to one formula rather than another, for the purpose of representing them; but that up to heights of at least 13,000 feet, a *linear* formula, or one which assumes the lowering of the boiling point to be exactly proportional to the height, seems to express the observations as well as any other; and the rate of diminution is almost the same as that deduced from Professor Forbes' observation, or a lowering of 1° for 538 feet of ascent.

The author has little doubt that M. Regnault's table, (which was not published when he last wrote), does really represent the law according to which water boils more accurately than the simpler linear formula, though the difference is in most cases insensible. For all ordinary heights (or up to 12,000 feet) Regnault's table may be more accurately represented by the formula

$$h = 535\ \mathrm{T}.$$

Where h is the height in English feet, T the lowering of the boiling point in Fahrenheit's degrees, reckoning from 212.° But he finds that Regnault's table may be represented in every case which can occur in practice, and with almost perfect accuracy, by the following formula, which is nearly as easy to use:—

$$h = 517\ \mathrm{T} + \mathrm{T}^2.$$

On the Chemical equivalents of certain Bodies, and the relations between Oxygen and Azote. By Professor Low.

The author commences his paper with a review of the opinions entertained by Dalton, Berzelius, and others, regarding the equivalent numbers of hydrogen, oxygen, nitrogen, and carbon, which have been differently fixed, according as we start from combination by weight or by volume. He remarked that while either view was perfectly suited to explain all the general phenomena of decomposition, yet since chemists had begun to examine the phenomena of substitution, it became apparent that it was absolutely necessary to employ the equivalents determined by weight. The author then proceeds to show that on a proper comparison of the properties of these elements, and of the constitution of their compounds, their atomic weights must be Hydrogen 1, Carbon 6, Nitrogen 7, Oxygen 8.

Reference is then made to the nature of azote, and to the opinion more than once expressed since its discovery in 1772, that it might be a compound, and to the views of Davy and Berzelius, the latter of whom supposed it must contain an inflammable base, which he proposed to term Nitricum. The author stated that he had long since arrived, by an entirely different line of argument, at the conclusion that nitrogen was a compound substance containing carbon; and as no other element can possibly combine with that substance so as to produce a compound whose equivalent shall be 7, except hydrogen, he concludes that azote is actually represented by the formula CH. Pursuing the same line of argument, he pointed out that oxygen might be a compound of azote and hydrogen, and referred to certain properties of ozone as indicating its compound nature. The author concludes his paper by showing how in all probability other elements might actually be considered as compounds, referring particularly to selenium and tellurium, chlorine, iodine, and bromine, and the metallic bases of the alkaline earths and alkalies.

Monday, Dec. 18, 1854. Right Rev. Bishop Terrot, V.P., in the Chair.

Miscellaneous Observations on the Salmonidæ. By John Davy, M.D., F.R.S., Inspector-General of Army Hospitals.

These observations are given in seven sections.

In the 1st, the author treats of the air-bladder of these fish, and the contained air, which he found, in every instance that he examined it, to be chiefly azote.

In the 2d, he points out a mistake he had fallen into in the instance of the female fish, as regards its abdominal aperture, which in a former paper he had described as open only for the passage of the ova; on further examination made on the larger species, he has ascertained, that though virtually closed, except during the spawning time, it is not absolutely either by a membrane or adhesion.

In the 3d, on the breeding localities of the Salmonidæ, he states his opinion that running water is not essential to the hatching of the ova, and he adduces instances in proof and illustration.

In the 4th, which is on the variable time of the hatching of the ova, he describes examples of difference as to time of the production of the young fish under circumstances apparently identical, or circumstances only very slightly different, tending to show the influence of a *vis insita* in the several ova.

In the 5th, on circumstances and agencies likely to take effect on the young fish, he notices two trials,—one in keeping the young fish in darkness after quitting the egg, which had no marked influence; the other in keeping them in the smallest portion of water capable of covering them, in relation to the position of young fish during a time of drought; in one instance life was protracted 52 hours; in another 74.

In the 6th, on the food of the young fish, he endeavours to prove that the food most suitable for them, and for which they are best fitted, is the infusoria. Young charr, under his observation, attained their perfect form and became fit to be set at large, to which no food had been given, and were, it is presumed, fed and nourished by these microscopic animalcules.

In the last section he submits some remarks on the vexed question of the par, viewed as a species, and comes to the conclusion that till a par is found propagating its kind, proof must be held to be wanting of the existence of such a fish, a true species distinct from the salmon or sea-trout fry.

On the Structural Character of Rocks. Part III., *Remarks on the Stratified Traps of the neighbourhood of Edinburgh.* By Dr Fleming.

The author referred in the first instance to the character of Stratification, illustrating the subject by specimens displaying the intermittent character of the carrying agent, and of the supply of material, pointing out the Hailes Quarry as furnishing the best example of the repetitions of strata. He then stated the views of Townson, Whitehurst, and Jameson, as to the relation of the trap rocks to the sandstones with which they are interstratified. He then took notice of a statement in the thirteenth volume of the Transactions of the Society, by Lord Greenock, that Edinburgh may be considered as a *valley of elevation*, the trap rocks in the neighbourhood dipping outwards as from a common centre. This opinion, he stated, was true in reference to the rocks on the east and west sides of the city, but not true as to those on the south and north, as at Blackford and Burntisland. Dr Fleming then stated, that there were nine masses of trap in the neighbourhood, included in the sandstones, all of them having some peculiar structural characters,—viz., Calton Hill, Salisbury Crags, Arthur's Seat, Lochend, Hawkhill, Blackford, Craiglockhart, and Granton. At this part of the paper he made some remarks in the so-called "outburst of trap" of Inchkeith, stating that the island consisted of at least a dozen of beds of trap alternating *regularly* with acknowledged sedimentary beds of sandstone, shale, and limestone, containing organic remains.

The author then commenced his survey of the stratified traps of the neighbourhood, by considering particularly the structural character of the Calton, or as it was termed at an earlier period, the Caldton. This trappean mass he considered as extending from Greenside to Samson's Ribs, including Heriot-Mount, St Leonard's, and the Echoing Rock. The Calton Hill had been described by Townson, Faugas St Foord, Jameson, Webster, Boué, Saussure, Cunningham, Milne, and Maclaren.

Dr Fleming then illustrated his views of the sedimentary character of the whole hill, by tracing on the Ordnance map, the coloured spaces occupied by the twelve beds of which the hill consists, assisted by a coloured section. The peculiarities of each bed in regard to its structure and mineral contents were pointed out; the author concluding by noticing the more interesting of the simple minerals of the hill, especially the Sarcite of Townson, first characterized from Calton specimens, and afterwards known as Cubizite and Analcime, exhibiting a specimen which he had procured from the hill when a student at the University.

SCIENTIFIC INTELLIGENCE.

ZOOLOGY.

Chlorophyll in Green Infusoria.—Prince Salm Horstmar has found that the green infusoria which form so abundantly on stagnant water, when treated with alcohol, give an extract having all the optical properties of a solution of chlorophyll. It gives the black-band in the red part of the spectrum described by Stokes, as well as dispersion of a blood-red light. The same result was obtained with an alcoholic extract of Spongia fluviatilis.—*Poggendorff's Annalen*, vol. xciii., p. 159.

Noctiluca Miliaris exists in the Mersey in myriads. It is this species chiefly which imparts a phosphorescent appearance to the water at night, as may be proved at any time by taking some of the river water containing them into a perfectly dark room, and splashing it about with any hard body to irritate them. They may be seen as little hyaline-globules about the size of a pin's head. Three or four years ago, in company with Mr Price, we saw millions of them collected together at Hilbre Island, in a little pool, when they tinged a portion of the water, about two yards in circumference, with a deep pink colour. The individuals in this collection were of a light pink hue under the microscope; those from the river are colourless. The men upon the ferry steamers state that the phosphorescent appearance of the water is much more noticed some years than others. They associate its presence with southerly winds.—*Byerley, Fauna of Liverpool, in Lit. and Phil. Trans. for* 1854.

Actinia Troglodites has been found in pretty good numbers upon the Leasowe shore and near Egremont slip. I have kept as many as eight or ten together for upwards of six weeks. They were often very ill-used for want of a fresh supply of sea water, but seemed to be most tolerant under the infliction. It was seldom until after having been kept for ten or twelve days in the same water, that they began to droop considerably, and they were speedily restored by a change. No food was given at any time. At first they threw off a great number of germs or ova, which, before they were extruded, could be plainly seen through the external envelope, and especially at the bases of those specimens which had not attached themselves, and could be turned over for examination. It appeared quite clear to me that these germs, young actiniæ, (or whatever they may properly be called), made their exit through breaches of continuity in the outer envelope, near its junction with the basal disk, and sometimes through ragged apertures in the base itself; in fact, I have hooked out the germs which were just on the point of emerging with a blunt probe, which was delicately used, and *did not make* the opening. The germs were about the size of a pin's head, and perfectly globular; they showed, by careful watching, a very sluggish motion. Three or four were put into a wide-necked 1½ oz. bottle, having a ground glass stopper, with some sea water, and were intended for a microscopic inspection in the evening; they were quite forgotten, however, and at the expiration of two months, one was found to have become developed into a perfect but very small actinia, the oral disk with the tentacles being fully and beautifully expanded. It is now (after six months) alive, but has never increased in size; it continues closely shut up, when there is a fresh supply of water, for some days, but after a week, and from that to a fortnight, fully expands again. For this reason the water has not been changed more than six times since it has been in my possession. No pabulum of any kind has ever been given. It seems to make no difference whether the stopper is kept in the bottle or not, so far as the animal's health is concerned. These creatures were shy of expanding during the day, and then were as flat as a coin. I used always to pay them a visit before bedtime, knowing that I should be repaid by a view of their full-blown expansion during the previous darkness; the stimulus of candlelight used to set their tentacula in active motion, without making them "retire for the night."—*Ibid.*

Testaceous Mollusca.—The following northern species of testaceous mollusca reach their most southern habitat about the northern and central parts of the British Seas, though a few of them re-appear on the Nymph bank, a kind of Arctic outpost off the south of Ireland.

Panopœa Norvegica, North Sea
Tellina proxima, „
Astarte elliptica, Clyde and North Sea
„ arctica, Zetland
Cardium Suecicum, Irish Sea
Crenella nigra, North Sea, Hebrides
„ decussata, „ „
Nucula tenuis, Scotland, Irish Sea
Leda pygmæa, Hebrides
Pecten niveus, „
Anomia striata, „
Hippothyris psittacea, North Sea
Terebratula Cranium, Zetland
Chiton Hanleyi, North Sea, Hebrides
Chiton marmoreus, N. Sea, Hebrides
Acmæa testudinalis, Irish Sea
Pylidium fulvum, Clyde & South of Ireland
Propylidium ancyloides, „
Puncturella noachina, „
Emarginula crassa, Carnarvonshire
Trochus Alabastrum, Orkney
„ undulatus, Hebrides
„ helicinus, Hebrides & Irish Sea
Scissurella crispata, Clyde
Aporrhais Pes Carbonis, Zetland
Cerithium metula, Zetland
Scalaria Grœnlandica, North Sea

Chemnitzia rufescens, Clyde
Natica helicoides, Orkney & North Sea
„ pusilla, North Sea
Velutina flexilis, „
Trichotropis borealis, South of Scotland
Fusus berniciensis, North Sea
Fusus Norvegicus, North Sea
„ Turtoni, „
Trophon clathratus, Irish Sea
„ Barvicensis, North Sea
Mangelia Trevilliana, „
„ nana, Orkney
Philine quadrata, North Sea

The following are northern species, extending only to the British channel, or but little to the south of it.

Xylophaga dorsalis
Mya truncata
„ arenaria
Thracia villosiuscula
Cochlodesma prætenue
Tellina pygmæa
Cyprina Islandica
Astarte compressa
Modiola Modiolus
Leda caudata
Megathyris cistellula
Chiton ruber
Lacuna pallidula
„ vincta
„ crassior
Rissoa Zetlandica
Skenia planorbis
Scalaria Trevilliana
Aclis nitidissima
Eulima bilineata
Natica Montagui
Buccinum undatum
„ Humphreysianum
„ Dalei
Fusus Islandicus
„ propinquus
„ antiquus
Mangelia rufa
„ turricula

Crenella discors, I have never met with south of the British seas, and suspect that when reported from the south of Europe, it has been confounded with *Crenella marmorata*, and *Crenella costulata*. Philippi's description evidently applies to the former.

* * * * * * * *

It is a most remarkable fact connected with the distribution of land shells, that some species are extended over very wide districts, while others are restricted to an area of a few square miles, or even less. Great Britain does not offer for observation a single species which is not likewise an inhabitant of France or Germany, though the neighbouring countries of the continent possess some which are not to be met with in this kingdom; and while thus among the hundreds of islands of Great Britain not one produces a species peculiar to itself, in the groups of the Canaries, Madeiras, and Azores, each island presents some species supposed to be strictly local.

This fact is particularly striking in the Madeiras—where Madeira proper contains but few species; while the small island of Porto Santo supplies an astonishing number, in general specifically distinct from those of Madeira, and the rocky islets called the Desertas, with difficulty accessible by man, have each some peculiar forms and in great abundance.

These facts seem to indicate that Great Britain and Ireland, including the Hebrides, Orkney, Zetland Islands, &c., have at one time formed part of the European continent, but that the more distant islands which I have named—raised by volcanic action from the depths of the Atlantic, have been each the scene of the creation of certain species which have been confined within their narrow limits by the surrounding sea.

Opposed to this idea is the fact already alluded to, that some marine littoral species, I may particularly mention *Littorina striata*, are common to West Africa, the Canaries, Madeira, and the Azores, which (as it is quite impossible for littoral phytophagous animals to have travelled along the bottom of the ocean) would lead us to infer that the African continent had at one time extended as far west as the last-named islands, in accordance with an opinion very ably supported by Professor Edward Forbes, in his report on the connection between the distribution of the existing Fauna and Flora of the British Isles, published in the Memoirs of the Geographical Survey of Great Britain. Which of these theories is correct, or whether they can both, with some modification, be reconciled to each other, I must leave for geologists to determine. The only solution which suggests itself to me is, that the shores of the African continent may have extended as far west as the islands in question, and that immediately on the subsidence of the land, when it was barely submerged, and the conditions not yet incompatible with the existence of littoral species of marine mollusca, the volcanic action took place, elevating the lofty masses of which most of these islands are composed, and that their peculiar land mollusca are of more recent origin.

Such an explanation would, I believe, be consistent with established geological facts, but I merely suggest it for the consideration of those who are more qualified than I can pretend to be to grapple with the vast subject of the history and conditions of our planet, in times anterior to the present distribution of land and water.—*Macandrew, in Proc. of Lit. and Phil. Soc. of Liverpool,* 1854.

GEOLOGY.

Action of Water and Air on Basalt.—Bensch having ground a quantity of basalt to a fine powder, with water on a porphyry slab, left it for some months in a beaker glass covered with paper. At the end of that time it was found to have been converted into a mass so hard as to require a smart blow of a hammer to break it. Its fracture was similar to that of the natural basalt, and the interior consisted of a black core, having a waxy lustre, and surrounded by a less compact gray mass. By longer exposure to the air, an efflorescence of carbonate of potash appeared on the surface, and 1·8 per cent. was extracted by water. The specific gravity of the basalt was 2·887, and after extraction of the carbonate of potash the internal portion of the altered basalt had a specific gravity of 2·1588; that of the external portion was 2·0423. There is no doubt that a hydrate must have been formed in this case, and the observation may serve to throw some light on the changes which take place in the weathering of rocks.—*Annalen der Chimie und Pharmacie,* vol. xci. p. 234.

Pleistocene Classification.—The following table of the classification of the different formations of the pleistocene or glacial period of geology, is constructed from Mr Smith's papers, and may help us to form an idea, or rather to lose ourselves in the attempt to form an idea of the extent of time necessary for its production.

1. Elevated marine beds. Ancient beaches.
2. Submarine forests.
3. Alluvial beds, most likely marine, but affording as yet no organic remains.
4. Upper Diluvium or Till. The most recent deposit of the Till. Has yielded bones of the fossil elephant, and water-worn shells. "Cyprina Islandica," "A balanus," &c.
5. Marine beds in the Till, affording shells. Occur at Airdrie 500 feet above the sea level. A bed of "Tellina proxima." In site under No. 4, and above No. 6.
6. Lower Diluvium, Till, or Boulder Clay.
7. Stratified Alluvium, consisting of sands, gravels, and clays, without organic remains. Resting in the Clyde district, immediately upon the upper members of the carboniferous system.—*Ferguson, in Proc. Lit. and Phil. Soc. of Liverpool,* 1854.

Observations on some Mines of the United States. By Dr Charles T. Jackson.—A long band of iron and copper pyrites exists in the State of Vermont, which has been long worked for the manufacture of green vitriol. In the districts of Vershire and Corinth, it becomes so rich in copper, as to contain on the average, 16 per cent. of that metal. Dr Jackson has examined the conditions under which the copper is met with. It occurs in a series of parallel veins situated between beds of mica slate, the direction of which is nearly north and south, and the inclination of which is about 30°. The mean thickness of the veins is from three to four feet. The pyrites contains a notable proportion of gold, though not in sufficiently large quantity to permit its separation at the cost of the copper. Dr Jackson proposes to obtain it by roasting with a quantity of nitrate of soda, lixiviating the sulphate of copper, and extracting the gold from the residue by amalgamation.

The most interesting mine in Vermont is that of Bridgewater, situated about five miles from the village of that name. The veins, whith are numerous, are quartzose, and contain gold, argentiferous galena, blende, and copper pyrites. The neighbouring rocks are talcose and chlorite slates, formed of granular quartz, with talc and chlorite in crystalline scales. The beds run from north-east to south-west, while the veins of auriferous quartz run nearly north and south. On examining the quartz veins, where they are cut by the stream which passes through the valley, numerous particles of gold were found. Blende and galena are the principal minerals of the vein; and on pulverizing and washing different specimens, there was always obtained a large quantity of galena mixed with gold, which can be separated without the use of mercury. The whole gold passes into the lead, and can be readily separated by cupellation. A ton of the lead gave in this way, gold to the value of 603 dollars, and 25 dollars of silver. It is remarkable that all the accessory minerals of the veins contain gold, and it is present in the gahnite and blende, which are found in them.

The mines of Georgia and North Carolina, are at present in active work, and their production is rapidly on the increase. The Goldhill mine in North Carolina produces weekly gold to the value of 3000 dollars.

CHEMISTRY.

Researches upon the Ethers. By M. Berthelot.—M. Berthelot has studied the action exercised by the acids in sealed tubes with the aid of time and heat, upon the compound ethers, common ether, and alcohol. This action, in certain cases, results in known phenomena, in others it has given some new results, which are not without some interest, relative to the constitution of ethers.

They belong to three different classes—

1st. The formation of compound ethers by means of common ether and acids.

2d. The direct formation of ethers by means of alcohol and acids.

3d. Decomposition of ethers under the influence of water and acids.

1. Formation of compound ethers by means of common ether and acids.

M. Berthelot obtained benzoic ether, by exposing benzoic acid and ether in a sealed tube to a temperature of 360° C. for nine hours; the compound thus obtained had the odour and all the properties of benzoic ether; it boiled at 210° C. The formation of this ether commenced at 300° C., but at this temperature even after prolonged contact very little was formed.

Ether and palmitic acid produce by heating for nine hours at 360° C.; palmitic ether, fusible at 22° C.

Ether and butyric acid at 360° C. in six hours produce butyric ether.

Ether and fuming hydrochloric acid by fifteen hours, contact at 100° C. form hydrochloric ether.

2. Direct formation of the compound ethers by means of alcohol and acids.

By heating in sealed tubes at a temperature of 250° C., alcohol and the fatty acids, combination readily takes place. In this manner the following compounds have been obtained.

Methylpalmitic ether, a crystalline compound fusible at 28° C., and solidifying at 22° C.

Ethylpalmitic ether, fusible at 21·5° C., and solidifying at 18° C.

Amylpalmitic ether, a waxy substance fusible at 9°.

The combination of alcohol with the fatty acids is never complete, either for the alcohol or the acid; but the formation of these three ethers is most abundant in the presence of excess of acid.

At 100° C. after thirty hours contact, benzoic, acetic, and butyric ethers are produced in great abundance. Stearic ether is produced in small quantity in about 102 hours; but the action is complete in this period when acetic acid is present.

3. Decomposition of ethers by the action of water and acids.

The formation of the compound ethers is never complete: this is owing to the decomposing action exercised upon them by the water set at liberty during the reaction. The presence of acids increases the intensity of this action upon the ethers.

Water heated to 100° C. during 102 hours with stearic and oleic ethers, begins to decompose them with the regeneration of stearic and oleic acids; but under the same conditions does not act upon benzoic ether.

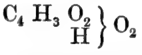
Water at 240° C., after some hours' contact begins to acidify benzoic ether; but the decomposition is feeble. Acetic ether, however, undergoes considrable decomposition at this temperature.

Acetic acid, diluted with two or three times its volume of water, by contact for 106 hours at 100° C., acidifies in a great degree stearic, butyric, and benzoic ethers, without producing any acetic ether.

Benzoic acid at 240° C., assists the decomposition of acetic ether, but only traces of benzoic ether, are formed, the greater part of this acid remaining free.

The acid which produces the decomposition may also enter into combination with the alcohol. The phenomenon is then nothing more than a simple replacement of one acid by another. The action of benzoic acid upon acetic ether is of this kind; with fuming hydrochloric acid the action is more marked; in 106 hours at 100° C. it produces decomposition with acetic, butyric, benzoic, and stearic ethers, setting these acids at liberty and forming hydrochloric ether.

Constitution of the Amides.—The researches of Gerhardt and Chiozza on the secondary and tertiary amides have led them to suppose that these substances are formed on the type of ammonia, in which one, two, or three equivalents of hydrogen are replaced by compound radicals. Wurtz takes a different view of their constitution, and supposes them to be formed like the acids on the type of water. Adopting the commonly received equivalents, acetic acid may be represented according to Gerhardt's view by the formula



$$\left.\begin{matrix} C_4\,H_3\,O_2 \\ H \end{matrix}\right\}O_2$$

Acetamide is formed from it, according to Wurtz, by the elimination of the two equivalents of oxygen which are placed externally to the group in combination with two equivalents of hydrogen, and the residue NH. come in their place. The different amides of acetic acid in this view would be represented in the following manner.

Acetic Acid.	Acetamide.	Ethylacetamide.
$\left.\begin{matrix}C_2H_3O_2\\H\end{matrix}\right\}O_2$	$\left.\begin{matrix}C_4H_3O_2\\H\end{matrix}\right\}NH$	$\left.\begin{matrix}C_4H_3O_2\\H\end{matrix}\right\}N(C_4H_5)$
Anhydrous acetic acid.	**Diacetamide.**	**Ethylodiacetamide.**
$\left.\begin{matrix}C_4H_3O_2\\C_4H_3O_2\end{matrix}\right\}O_2$	$\left.\begin{matrix}C_4H_3O_2\\C_4H_3O_2\end{matrix}\right\}NH$	$\left.\begin{matrix}C_4H_3O_2\\C_4H_3O_2\end{matrix}\right\}N(C_4H_5)$

The amides of a bibasic acid may be similarly represented, and selecting oxalic acid as an example, we have the following formulæ.

Oxalic Acid.	Oxamide.	Oxamic Acid.	Oxamethane.	Diethyloxamide.
$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}O_2$	$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}NH$	$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}NH$	$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}NH$	$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}N(C_4H_5)$
$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}O_2$	$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}NH$	$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}O_2$	$\left.\begin{matrix}C\ O\\C_4H_5\end{matrix}\right\}O_2$	$\left.\begin{matrix}C_2O_2\\H\end{matrix}\right\}N(C_4H_5)$

The production and character of the amides are readily explained according to this view, but though ingenious it can scarcely be considered as equal in simplicity and beauty to that of Gerhardt's. The principal, indeed, the only advantage it possesses is that it affords an explanation of the feebly acid properties of some of the amides, in so far as the basic hydrogen of the original acid is not removed. On the other hand, if we carry it out to its full extent, we should expect all the amides to possess acid properties, which they certainly do not; nor is there any reason why oxamic should not, like oxalic acid, be bibasic, for it would still contain two equivalents of basic hydrogen.—*Annales de Chimie et de Physique.* 3d Series, vol. 42, p. 43.

Alcohol from the Tubercules of Asphodelus ramosus.—The tubercules of Asphodelus ramosus have been employed for some years in Algeria for the manufacture of alcohol. It has been asserted that they contain neither starch nor sugar, and the experiments of M. Clerget fully confirm this opinion. When grated and pressed they yield 81 per cent. of juice of specific gravity 1·082. When treated with iodine not the slightest indication of starch can be obtained. The juice has no action on polarised light, but if it be heated with hydrochloric acid at the boiling temperature it rotates the plane of polarisation to the left very powerfully. When mixed with two per cent. of yeast it enters rapidly into fermentation, and yields 8 per cent. of alcohol, being about twice as much as can be obtained from the juice of the sugar beet. The dried tubercules of the plant do not yield more than 3 per cent. of alcohol. M. Clerget is engaged in the investigation of the principle which undergoes fermentation.

BOTANY.

On Datura Stramonium.—M. Alphonse De Candolle has made observations on the origin of Datura Stramonium, the thorn apple, and other allied species, in which he states—1. Datura Tatula, L., is in all probability of American origin, being a native of Venezuela, perhaps of a large portion of South America, and of Mexico; it might have been imported into Europe about the sixteenth century, and have thus become naturalized first in Italy, then in the south-west of Europe, without having as yet reached the south-eastern part. 2. Datura Stramonium, L., appears to have been a native of the Old World, probably of the borders of the Caspian Sea and the adjacent regions, certainly not of India; and it is very doubtful whether its existence in Europe can be traced back farther than the time of the Roman Empire; it seems to have been scattered over Europe between that epoch and the discovery of America.

Datura ferox, L., is a very doubtful plant, both as regards the species and its native country. It seems to be a variety of Datura Stramonium.

Datura Metel is easily distinguished by its pubescence and its reflected fruit, but its native country is also doubtful. It seems to be indigenous in intertropical America.—*Bibliotheque Univ. de Genève*, Nov. 1854.

New Himalayan Genera.—The following are two of the most remarkable new genera that have hitherto presented themselves to us during the examination of our Indian Herbarium. Their very remarkable structure has induced us to take the earliest opportunity of making them known, believing, as we do, that they are peculiarly interesting both in a structural and systematic point of view. The genus *Maddenia*, in particular, is quite exceptional in its order, from presenting apparently normally dimorphous flowers, a feature that has not hitherto been recorded amongst *Rosaceæ.*

Diplarche, which is an undoubted Ericeous plant, differs from the majority of the family in the longitudinal dehiscence of the anthers, and from all in the two series of stamens, of which the outer or upper series is epipetalous, and the lower sometimes epipetalous, but more frequently hypogynous.

In the name *Maddenia* we are desirous of commemorating the botanical services of Major E. Madden, of the Bengal Artillery, a well-known and most valuable contributor to our knowledge of Himalayan plants.

We have named the genus *Diplarche,* in allusion to the two series of stamens, which is its most remarkable character. Its nearest affinity is certainly the little *Loiseleuria procumbens* (*Azalea*, Lin.) of the Scottish mountains, which is also a native of the Arctic regions, and of the alps of Northern and Southern Europe, Siberia and North America, but does not inhabit the Himalaya. With this, *Diplarche* agrees in habit, and in the dehiscence of the anthers, but differs in the alternate leaves, and many other important characters of inflorescence and flower. The dehiscence of the capsule is normally septicidal, though not obviously so at first, owing to the dorsal portion of the valves breaking away from the septa, which remain attached to the axis of the capsule as thin scarious membranes. The ripe capsule appears to have two integuments, the outer coriaceous coat of each valve separating from the inner or more crustaceous one, whose margins alone are inflexed.

It has been remarked long ago, by De Candolle and others, that *Ericeæ* are intermediate between *Calycifloræ* and *Corollifloræ;* and though the present genus certainly tends to favour this view, it does not in our opinion throw any further light upon the position of the great order, or rather alliance, of *Ericeæ.* These great groups of Jussieu are no doubt, to a great extent, artificial, but in the present state of systematic botany they are essential aids to determining the positions of the many Natural Orders they include: for this purpose we believe them to be the most valuable that have been suggested hitherto.—*J. D. Hooker and T. Thomson, in Hooker's Journal of Botany*, Dec. 1854.

Plants in the Crimea.—In the "Gardener's Chronicle" for December 16, 1854, the following account is given of some of the vegetable productions of the south-western portion of the Crimea, and their existence seems to indicate a winter climate not more severe than that of Hampshire or Sussex.

Every gardener knows that in hard winters, even near London, Cistuses of all kinds are killed; but we learn from Marschall v. Bieberstein that *Cistus creticus* is not uncommon (minimè rarus) on the hills of the S. Crimea overlooking the Black Sea. Pallas also relates that the Manna Ash, a tender tree near London, inhabits the warm southern dales; therefore the winters to which these are exposed cannot be worse than those round London. The same remark applies to the dyer's Sumach, *Rhus*

Coriaria, which forms trees in the southern valleys, and which entirely justifies the inference we formerly drew from the presence of *Pistacia Terebinthus*, a tender tree common in the South Crimea, where it forms a trunk as thick as a man's body. We have lately heard the accuracy of our statement about the Caper plant questioned, and doubts expressed as to whether it really grows in the Crimea at all. We, therefore, beg to quote the words of our auththority as to "Perfrequensin is sterilibus subsalsis Tauriæ, ad pontum Euxinum, et in planitiebus caspico-caucasicis. Colliguntur Capparides ab incolis oppidi Kisljar et per omnem Rossiam divenduntur." (*Bieb. Fl. Taur. Cauc.*, II., 2.) This is said of the sharp-leaved variety of *Capparis spinosa*, called *ovata*. Pallas also mentions the Caper bush, and says it is called Shaitan-Karbus.

Pallas enumerates as many as 24 distinct varieties of grapes cultivated either for wine or the table. Some of these are no doubt European varieties introduced, others are not recognisable as such. None of them appear to be of importance enough to deserve cultivation in this country. Far otherwise is it with the apples of the Crimea, of which we hope that some of our officers will be able to secure cuttings when the fatigues of their campaign shall be over. Pallas speaks of one called Sinap-Alma, which keeps till July, and only acquires its excellence before the new year. Of this we are told that waggon loads are annually sent to Moscow and even to St Petersburgh. There is also an autumn apple, which a friend, who was on the south coast in 1847 or 1848, thought by far the best he had ever tasted in any country.

We must not omit, in taking our farewell of the Crimean climate, to mention the existence of a cobnut of extraordinary size, for a few specimens of which we are indebted to Captain George Elliot, R.N., of H.M. ship Arethusa, who obtained them at Eupatoria. Pallas calls them Trebizond-Funduk, describes them as "short obtuse nuts of uncommon size," and says that they are the produce of *Corylus Colurna*. What we have received are larger than any that we have before seen.

MINERALOGY.

Artificial Production of Silicates and Aluminates. By M. DAUBREE.—By bringing chloride of silicium and other volatile chlorides in contact with lime and other bases at a red heat, decomposition occurs, and silicic acid is produced and is deposited in crystals, either alone or in combination with the bases present. By means of lime, magnesia, alumina or glucina, and chloride of silicium, crystallized quartz is obtained in its usual form, and part of the base is converted into a silicate. With lime Wollastonite (table spar) is obtained in rhombic tables, with two faces replacing the obtuse angles, exactly as in the natural crystals. These tables are frequently united in the form of a cross, like the crystals of staurolite. By means of magnesia peridote is obtained, in rectangular prisms. Alumina gives a silicate in long prisms with an oblique base, which is not attacked by acids, is infusible, and has all the properties of kyanite. It is interesting to observe that in this reaction chloride of aluminium is produced at the cost of the silicium.

In order to produce a double silicate, it is not enough to mix with two bases in the requisite quantity, but there must be an excess of one of them in order to supply the requisite amount of oxygen to the silicon. In this way a mixture of lime and magnesia yields colourless and transparent crystals of augite (diopside). By a mixture of seven equivalents of potash or soda, and one of alumina, or one of alkali, one of alumina and six of lime, crystals of the form and characters of felspar are obtained. By using different bases, and modifying their proportions, crystallized Willemite (silicate of zinc), idocrase, garnet, phenakite, emerald, euclase, and zircon are obtained. By making a mixture corresponding to the constituents of magnesia, tourmaline, and iron, and magnesia tourmaline, adding excess of lime or magnesia, and exposing the whole to the chloride of silicium, in addition to rock crystal, very distinct hexagonal prisms with all the properties of tourmaline were obtained.

By passing chloride of aluminium over red-hot lime, crystals of alumina, corresponding to the two well-known forms of corundum, were obtained. When magnesia is used, the silicic acid unites with the excess, and crystals of spinelle are produced. A mixture of chloride of zinc and aluminium, brought in contact with lime, produces gahnite.

Chloride of titanium, acting on lime, produces titanic acid in the form of Brookite. Chloride of tin gives the crystallized oxide. Chloride of iron gives specular iron ore, and if mixed with chloride of zinc, Franklinite is produced. Chloride of magnesium gives crystallized magnesia, exactly similar to the periclase of Monte Somma.

The results of these experiments lead to many interesting conclusions. They shew us how such minerals, as augite, garnet, epidote, axinite, and many other minerals, which certainly cannot have been produced by fusion, may be formed. Indeed, the production of a large number of minerals may, with great probability, be attributed to the action of volatile chlorides and fluorides, and the penetration of those into the fissures of limestone; and the very powerful action of lime on these compounds, may explain the abundance of silicates which exist disseminated through many limestones. Minerals, such as spinelle, chondrodite, mica, augite, amphibole, serpentine, &c., are frequently found in limestones which contain no magnesia, and this hitherto unexplained fact may be due to the difference in the chemical affinities of lime and magnesia; for it is observable that in all these experiments chloride of magnesium is decomposed by lime. Many other obscure facts may also be explained by reference to these researches, which are of very great mineralogical interest.—*Comptes Rendus*, vol. xxxix., p. 135.

Meteoric Iron from Greenland.—Forchammer describes a meteoric stone discovered by Rinck, in possession of the Esquimaux at Niakoruak, Lat. 69° 25′, by whom it had been found at a short distance from their hut, on a stony flat through which the river Annorritok flows into the sea. It weighed 21 lbs. The specific gravity of the whole mass was 7·00, that of small fragments varied from 7·02 to 7·073. It was so hard that it could neither be filed nor sawed, but was very brittle. Its fracture was granular; it took a high polish, and showed beautiful Widmannstatt's figures when acted on by nitric acid. By treatment with acids it evolves sulphuretted hydrogen, and hydrogen of bad odour exactly like inferior cast iron. At first iron alone is dissolved, and a black matter consisting of minute crystals is left behind, which eventually dissolves, and a black powder, which proved to be carbon, floats through the fluid, while, in place of the fragment of the iron, a gray porous mass amounting to 1 or 2 per cent. of the stone is left. It contained—

Iron	93·39
Nickel	1·56
Cobalt	0·25
Copper	0·45
Sulphur	0·67
Phosphorus	0·18
Carbon	1·69
Silicon	0·38
	98·57

Besides these there are found metals of the alumina group (with oxides soluble in caustic alkalies), of the Zirconia group (with oxides insoluble in alkalies, but precipitated from their salts by sulphate of potash), and of the Yttria group (oxides insoluble in alkalies, soluble in carbonate of ammonia, and not precipitated by sulphate of potash). The two latter groups, which have not been previously found in meteorites, form the principal part of the undissolved gray porous mass, but their quantity is so small that the author has been unable to determine with certainty what members of these groups are present.

The crystalline grains, which are less soluble than the rest of the mass, consist of iron and carbon, with small quantities of sulphur and phosphorus. Although it is difficult, if not impossible, to stop the solution at the proper point, so as to insure this substance being pure, Forchammer has made two analyses, and found 11·06 and 7·23 per cent. of carbon. A carbonate of iron having the formula $Fe_2 C$, would contain 9·66 per cent. of carbon, and this is probably its constitution. Its specific gravity is 7·172.

This meteoric iron belongs to a very rare variety, and contains so large a quantity of carbon, that it may be called meteoric cast-iron. That found in Greenland by Parry, as well as another specimen mentioned by Forchammer were perfectly malleable.—*Poggendorff's Annalen*, vol. 93, p. 155.

Analysis of some Minerals. By Carl Von Hauer.

Delvauxite.—The analyses of this mineral differ greatly from those given by Delvaux. Hauer finds a much smaller quantity of water and only traces of carbonic acid. His numbers for the air-dried mineral are—

	I.	II.
Silica	2·08	1·24
Lime	7·07	7·39
Peroxide of iron	46·40	46·34
Carbonic acid	trace	trace
Phosphoric acid	18·67	17·68
Water	26·04	26·71
	100·27	99·36

No. 1, from Berneau, Belgium; No. 2, from Leoben in Styria.

The mineral when dried over chloride of calcium, lost 9·02 and 9·92 per cent., and abstracting this quantity of water and the silica, which is obviously a fortuitous constituent, the results stand thus,—

	I.	II.
Peroxide of iron	52·03	52·54
Lime	7·94	8·37
Phosphoric acid	20·93	20·04
Water	19·08	19·04
	99·98	99·99

Leading to the formula $2(CaO)\ PO_5 + 5\ (Fe_2\ O_3)\ PO_5 + 16\ HO$, which is analogous to that given by Berzelius for the uranite of Autun, $2\ (CaO)\ PO_5 + 4\ (Ur_2\ O_3)\ PO_5 + 15\ HO$.

Kakoxene.—The specimen analysed consisted of silky needles and rounded masses, and also of dirty green kidney-shaped forms resembling wavellite. The former only were analysed.

Insoluble in hydrochloric acid	3·63
Peroxide of iron	45·05
Lime	trace
Phosphoric acid	18·56
Water	30·94
	98·18

This analysis agrees very closely with that of Richardson, and also with those of Steinman, provided we subtract the silica and alumina found by these chemists, and which are obviously impurities. The formula is $2\,(\mathrm{Fe_2\,O_3})\,\mathrm{PO_5}+12\,\mathrm{HO}$. The greenish kidney-shaped masses were of different composition, and their analysis leads to the formula $3\,\mathrm{Fe_2\,O_3}\;2\,\mathrm{PO_5}+20\,\mathrm{HO}$, but the author does not venture to describe them as a different species.

Gieseckite.—A very pure specimen from Nunasoruaursak in Greenland, gave—

Silica	46·40
Alumina	26·60
Peroxide of iron	6·30
Magnesia	8·35
Oxide of Manganese	trace
Potash	4·84
Water	6·76
	99·36

For this the author gives the formula—

$$3\left\{\begin{matrix}\mathrm{Mg\;O}\\ \mathrm{Fe\;\;O}\\ \mathrm{K\;\;\;O}\end{matrix}\right\}\mathrm{Si\,O_3}+2\,\mathrm{Al_2\,O_3}\;3\,\mathrm{Si\,O_3}+3\,\mathrm{HO}.$$

Anauxite.—A pure specimen of this mineral from Bilin, gave—

Silica	62·20
Alumina	23·82
Lime	1·00
Protoxide of iron Magnesia	traces
Water	12·40
	99·42

The formula is $\mathrm{Al\,O_3}\;3\,\mathrm{Si\,O_3}+3\,\mathrm{HO}$, which is that of cimolite, but although a similarity exist in constitution, there is none in mineralogical characters, for anauxite is found in minute crystals, while cimolite is amorphous. Breithaupt places it close to pyrophyllite, which however contains much less water.

[Several valuable communications have been received, which form want of space are unavoidably postponed to next number.]

THE

EDINBURGH NEW

PHILOSOPHICAL JOURNAL.

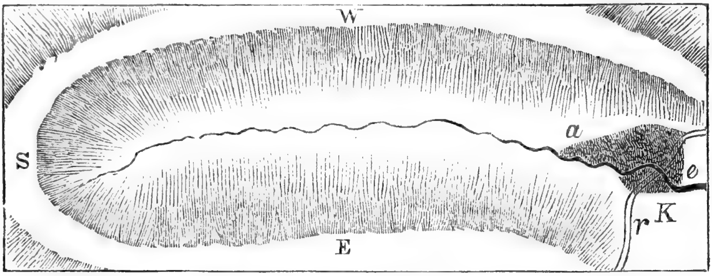
Notice of Ancient Moraines in the Parishes of Strachur and Kilmun, Argyleshire. By CHARLES MACLAREN, F.R.S.E.

Glaciers exert a powerful action on the face of a district which they have at any time occupied, and in this way traces of their ancient existence may be discovered long after they have disappeared. The most conspicuous of these traces are of three kinds; *first*, the striated, grooved, and dressed surfaces of the rocks over which the glaciers have moved; *secondly*, the piles of gravel and sand which collect on their sides and at their lower ends, and are called moraines; and, *thirdly*, the large blocks which they have transported to vast distances from their original localities, which blocks, indeed, once constituted part of the moraines, but are sometimes found unaccompanied by anything in the regular form of a moraine. Traces of all these three descriptions are met with in many parts of Scotland. Those now to be described are in Argyleshire.

Moraines in Glensluan.

Glensluan is situated about a mile southward from the village of Strachur. It runs nearly south and north, and is divided from Loch Fyne by a single ridge, a mile in breadth. The glen is about two miles and a half in length, fully two-thirds of a mile in width, and is inclosed on all sides by mountains, from 800 to 2000 feet high, except on the north, where it opens into Glen Eck. An unbroken circular wall of rock shuts in the south end S, forming the upper half of the glen into a very perfect amphitheatre. The north end N, is rather narrower, owing to the smaller elevation of its sides, and the whole when mapped, has a slightly curved form, the bearing of the upper portion being one point (or 11°) east of north, and that of the lower, two points. It is a trough, excavated in the mica slate, and conforming in its direction to the strike of the beds (see the cross section, Fig. 2), so that the streamlets which descend from the eastern ridge E, flow over the faces of the strata, while those descending from the western ridge W, flow over their edges. In consequence apparently of this dip of the rock to the west, the east side of the glen has in section a slightly convex, the west a slightly concave outline, and the latter is more highly inclined than the former. Both sides have a pretty thick coat of clay sand and gravel, which supports a strong growth of coarse grass, rushes, ferns, and heather. Scarcely any rock is seen, except in the beds of the rivulets, or occasionally on the west side at *t*, 400 feet above the bottom of the valley, where the edges of some of the beds of slate protrude, in consequence perhaps of their greater firmness. On the same side, but lower, and towards the foot of the glen, a bed of blue compact limestone crops out and is quarried. The Sluan, a rapid stream, running from south to north, discharges the collected waters of the glen into the river Cur near K, which river flows here through a flat meadow, and falls into Loch Eck, about two miles eastward.

Fig. 1.—Glensluan.



The bottom of the lower end of the glen for about 1800 feet from the meadow at K, is occupied by a remarkable series of mounds of clay and gravel, crossing the hollowlike embankments, and which, if met with in a valley of the Alps, even where no ice was visible, would at once be recognized as the terminal moraines of an ancient glacier. They are very conspicuous, and attract the eye of the traveller as he passes along the road, not only by their forms, which are peculiar, and strange for the situation they occupy, but by the contrast which their smooth surfaces, covered with bright green grass, and furrowed by the plough, present to the dark shaggy uncultured declivities amidst which they stand. These mounds had evidently, at one time, extended completely across the glen, but the stream whose course they barred, has cut a narrow passage for itself, not in the middle of the hollow, but close to the eastern ridge. Their external form, like that of the alpine moraines, is very irregular. Sometimes they present an arched outline across the valley, as in Fig. 3. The upper mounds *a* and *b*, Fig. 4, have this shape. Sometimes the two extremities are high, and the middle low; but in passing over the ground, it is easy to discover that since the materials were deposited, the surface has been much altered by the denuding action of the river itself, and the various streamlets which flow down from the western ridge after heavy rains. No section can give a correct idea of the whole deposit; and as one along the middle must have been to a great extent imaginary, I have thought it my best course to sketch in Fig. 4,

Fig. 2.

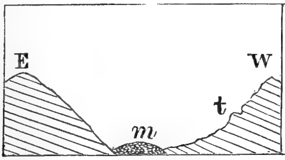


Fig. 3.

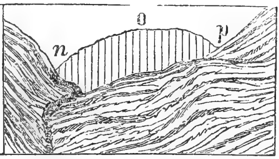
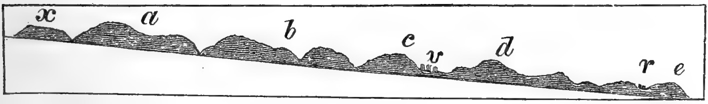


Fig. 4.



the natural section afforded by the deep cut which the river has made in its eastern side, where the composition of the mounds is well exposed, and their approximate depth seen.

The section, Fig. 5, passes right along the middle of the valley. S is the wall of rock which forms the southern boundary of the valley; M the moraines; K the meadow at the north end of the valley.

Fig. 5.

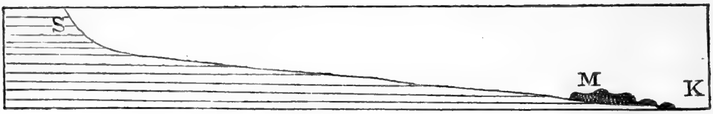


Fig. 2 is a section across the valley, showing its form; the oblique lines show the dip of the strata, which is to the west at an angle of 10° or 12°; *m* shews the position of the moraines, and *t* marks the place where portions of the rock, probably harder than the rest, protrude through the covering of turf. On the east side, as already mentioned, the strata present their sides, and on the west their edges to the surface, and this is apparently attended with a difference in the distribution of the alluvial matter.

Fig. 4 is a section along the lower part of the channel of the stream, showing the depth of the moraines as laid bare by the action of the water; *a*, the uppermost mound, has its surface clothed with grass, and marked by the ploughshare. At the river side, it presents an escarpment of clay and gravel, varying from 20 to 70 feet in vertical depth, but the height from the rock to the convex top is fully 100 feet. Its breadth across the valley is about 250 feet, and must have been 350 before the river channel was excavated.

The second mound, *b*, is divided from the first by a small ravine, cut by a streamlet; its top is about 15 feet lower than the top of *a*; its height above the stream fully as great; it is a little broader, and it is fully 500 feet in length, from south to north. Fig. 3 is a view of it taken from a point a little below the village. Its top, *n*, *o*, *p*, and its northern declivity, were beautifully green in November last, and, being divided into well-marked *riggs*, must have been under the plough very recently. The riggs are rudely represented in the figure by vertical lines. The dark crooked space below *n*, is the water course.

The elevations *c* and *d* have not the regular form of mounds or embankments, like *a* and *b*. They are rather detached hillocks, the remnants apparently of larger masses. Their height at the river varies from 20 to 40 feet; they are partly covered with wood, and are connected with the western hill, by grassy slopes of the same materials, which have been furrowed by torrents.

The last of the hillocks, *e*, is about 40 feet above the meadow in Glen Eck; *v* marks the site of the village, and *r* the road.

The entire mass of materials constituting these mounds, hillocks, and slopes, is spread over an area about 1800 feet in length. The breadth, including the river-bed, which has been carved out of them, is about 350 feet at the upper end, and 500 or 600 at the lower. The depth varies from a few feet to 100, and the whole form one continuous mass. The height of the first mound *a* above the meadow at K, I found by measurements taken with a pocket level, but not with all the accuracy I could desire, to be about 205 feet. The descent is more rapid here than in the upper part of the valley, as is well shown by the river channel.

In materials, form, and position, these mounds have precisely the character of the terminal or frontal moraines of glaciers at the foot of alpine valleys. First, as to the *materials;* they consist of the debris and detritus of the mica slate, in the form of sand, clay, and gravel, without any trace of stratification, but mixed with blocks. The blocks, which are partly angular, partly rounded, may be seen embedded in the masses where the interior is exposed, and a few are found on the surface, the remnant probably of a much greater number which may have been removed to clear the ground for the plough, or to build the cottages and outhouses of the village. Secondly, *in outward form* there is the same resemblance. A terminal or frontal moraine consists of sand, gravel, and blocks, carried down from the upper part of a valley by the ice, and deposited in piles or ridges at the foot of the glacier. When the glacier is advancing it pushes these before it; when retreating it leaves them behind it; and if it continues to retreat for a series of years, a succession of such accumulations is found at its lower end, generally in the shape of mounds or ridges, transverse to the direction of the glacier valley, sometimes in contact with one another, sometimes standing apart. Four or five ridges of small size may be seen at the foot of the upper glacier of Grindelwald, one behind another. There are three or four ancient ones in the valley of Lutchen, not far from Interlaken, forming a continuous mass, nearly a mile in length, of great depth, and with an undulating surface, like those in Glensluan. The glacier of the Rhone in 1826 had nine terminal moraines, one behind another, which Mr Desor, with reference to their form, calls (*digues*) embankments. In the Vosges mountains, where traces of ancient glaciers abound, the descriptions and sections of the moraines given in the work of Mr Collomb, would apply very accurately to those in Glensluan. Lastly, in *position*, the mounds of Glensluan have the same correspondence with the frontal moraines of glaciers; that is, they stretch across the foot of a valley, which, if it existed in the Alps, at the proper elevation, would contain a glacier.

Long valleys open at both ends, and nearly level, are not favourable to the existence of glaciers, which, it must be remembered, are *moving* masses of ice. There should be a cavity above to collect the snow and ice, and a certain fall in the ground to give it motion. The cavity is generally wider than the glacier-valley, and has received the names of "Reservoir," "Amphitheatre," "Basin d'Alimentation," and when very large, "Mer de Glace." The upper part of Glensluan, which is somewhat wider than the under part, is a very perfect amphitheatre, well fitted both to store up the materials of a glacier, and to set them in motion. Professor Forbes has shown, that the motion of a glacier is that of a semifluid mass, whose mobility increases with its breadth and depth; and when we find that even a valley, like Glen Eck, open at both ends, and with its bottom nearly level, has been the scene of glacier agency, as demonstrated by its powerfully abraded and grooved rocks, we cannot doubt that the same agent would act with still greater effect in Glensluan, which has, in the first place, a rather highly inclined bottom; and, in the second, has a wall of rock nearly vertical, at its head, to serve as a *point d'appui*, and urge the gravitating mass northward. With regard to the other traces of glacial agency, I saw distinct marks of abrasion on the protruding rocks of the western declivity, and I have little doubt that striated surfaces may be found, when more carefully sought for; but from the position of the strata, in reference to the direction of the valley, we cannot expect them to be numerous. Striæ and groovings are most deeply cut and best preserved on schistose rocks, when the line of glacier motion runs across the edges of the strata, while in Glensluan it coincides with the strike.

In the form, then, of these mounds, in the materials composing them, and in the position which they occupy, we have all the essential features of the terminal moraine, and in the valley above, we have the mould in which the ancient glacier was cast, and in which a glacier would certainly be again found, were the necessary climatic conditions to recur.

If we reject the glacier hypothesis, the existence of these mounds cannot be accounted for. The small stream of the valley has done something to destroy them, but neither it nor a much larger stream could possibly produce them; and the effect of debacles, oceanic currents, and similar cosmical agents, in whatever way employed, would be, not to raise such objects, but to level them. These oddly-shaped and oddly-placed piles of gravel must have remained a puzzle, if the icy regions of the Alps had not furnished a key to explain them.

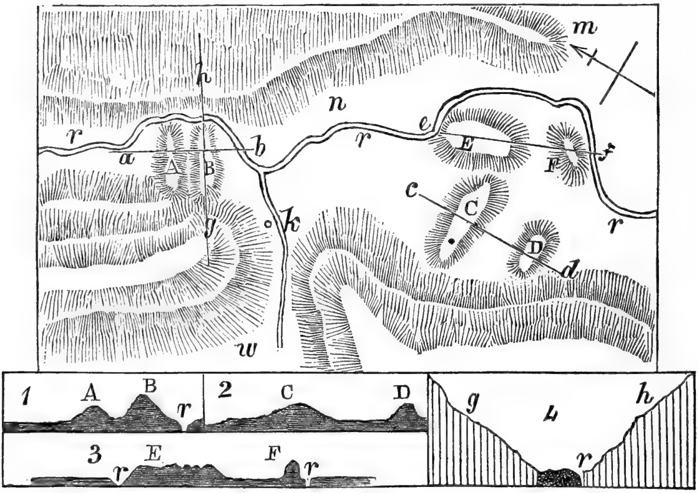
Immediately above the upper mound *a*, in Fig. 4, a smaller one, *x*, will be observed. It is the lower end of a ridge of the same materials (gravel and clay) which ascends from two to three hundred feet on the western declivity. It projects two or three yards from the surface of the hill at the head, and thickens to 30 or 40 feet at *x*, the foot. It has a large conspicuous boulder resting on its upper end, and there is a similar ridge running parallel with it, immediately above it in the valley. They are probably portions of a lateral moraine, passing here into a terminal one.

Glensluan joins Glen Eck, and it is possible that the lowest portion of the mounds may belong to the latter. Similar deposits of gravel and sand are found on both sides of Glen Eck. They are peculiarly abundant from Whistlefield to Strachur, and, though not in the distinct form, are probably the remnants or wrecks, of lateral moraines. At the junction of the two glens, the materials would be mingled, and it may be difficult to draw the line that separates them. There is no ambiguity, however, in the mounds above the village, which unquestionably belong to Glensluan.

Moraines in Glenmessan.

Ancient moraines of an equally remarkable character are found in Glenmessan, about three miles north-west from the village of Kilmun. This glen is a rugged, straight valley, two miles long, inclosed between mountains 1500 feet in height. At its head, or northern end, it is joined by two lateral valleys; and here it is nearly level and comparatively wide, while its southern portion is extremely narrow, and descends rapidly. The river Messan, which flows through it, is a considerable stream (*r*, *r*, *r* in the map below), and has a pretty waterfall. At the farm-house of Corusk (*k* on the map), the glen terminates in a plain from two to five furlongs in breadth, and which extends, with some inequalities, to the head of Holy Loch. Precisely at this southern termination, where the glen is still very narrow, it is crossed by two large mounds of earth (A, B). In shape they closely resemble the artificial embankments made for railways, or the dikes thrown across valleys to form reservoirs, or the ramparts constructed for military purposes to fortify or close up a moun-

Fig. 6.—Glenmessan.



tain pass. As any of these uses is quite irreconcileable with the position they occupy, the excursionists from Kilmun and Dunoon, who pass them on their way to the waterfall a mile farther up the valley, must have been sorely perplexed to account for their origin. Their great size demonstrates that they must owe their birth to some powerful agent. The elevation of the larger one, B, above the bank of the stream on its south side, as measured by a pocket level, I found to be 77 feet. The elevation of A, above the hollow which divides it from B is 40 feet. (See Section 1, below the map, which gives the profile along the line *a*, *b*; *r* is the river.) The length of B across the valley, in the direction *h*, *g* (see Section 4), is 320 feet; but it is truncated at the east end by the passage *r*, which the river has cut, and its original length must have been 350 feet, which is the present length of A. Both mounds are covered with herbage, but the truncated end of B discloses the nature of their materials, which is seen to be a confused assemblage of gravel and sand, with a few blocks of the mica-slate intermixed. Admit that they are ancient moraines, and every difficulty connected with their existence here disappears; and we shall seek in vain for any other rational explanation of their origin. Moreover, other evidence of the presence of mighty masses of ice abounds in the valley. Marks of abrasion may be seen on the contorted laminæ of the mica-slate to the height of some hundred feet; projecting ledges of the rock have their northern faces smoothed, while the southern remain rough, showing the direction in which the ice moved; and a highly inclined or nearly vertical surface, 10 feet high, just opposite the truncated end of B, and within 30 feet of it (under *h* in the map), is beautifully marked with horizontal grooves, from half an inch to an inch in breadth. There are other fine specimens of striation and grooving on the rock at a greater height, and the perfect condition in which these traces of glacial action are preserved is readily explained by the *planes of stratification being at right angles to the line of the valley*, so that the gliding mass of ice would pass right *across* the edges of the strata. It is always under such circumstances that the striæ and groovings are most distinct. When a glacier moves *along* the planes of a schistose rock, it will smooth the exposed surface if armed at its bottom with sand, but if armed with pebbles, it will tear off the laminæ, instead of cutting furrows in them.

At Stranlonick, more than a mile further up in the glen, there is a flattish mound of gravel and clay with blocks, which is probably also a terminal moraine, though less distinctly characterized than those just mentioned. It lies in the middle of the valley, having diverted the river to the east side. It measures about a furlong in length and breadth, rises from 10 to 30 feet above the stream, has a very uneven surface, with a few blocks on it, and many dispersed through its mass.

As already stated, a plain or meadow extends southward from B, and in looking over this plain, the eye is arrested by four very distinct mounds or hillocks, C, D, E, F, which are probably remnants of one or more frontal moraines. Their profiles will be seen in sections 2 and 3 along the lines *c d*, and *e f*. Mound C is about 600 feet in length; its greatest breadth 250; its greatest height 25 or 30 above the plain, into which it descends with gently sloping sides. (See section 2.) A boulder of mica-slate, measuring three cubic yards, rests on its west end. Mound E is 400 feet long and 200 broad, rises with abrupt sides 30 feet above the plain on the west side, and 40 on the east. An incision made by the river in its north end (*r*, section 3) shows that it is composed of sand, gravel, and blocks, without any trace of stratification, and on its uneven top there are the remains of a small plantation. Mound F is 200 feet long, very narrow, rises 30 feet above the plain on the north side, 40 on the south. (See section 3). Mound D is 210 feet long, 140 broad, and 30 in height above the plain. It has two considerable blocks resting on it. Both D and F are rendered picturesque by their sharp, well-defined forms, and the tufts of brushwood which crown them. The materials of all the six mounds are apparently identical, a confused assemblage of sand, gravel, and blocks; and that of the plain itself, as seen in the watercourse, seems much the same.

But if these mounds be fragments of moraines, the glacier which formed them must have rested on the gravel and clay of the plain, and must have glided over them, as it glided over the rock in the narrow valley above AB. Have we any proof of glaciers travelling over a bed of such materials? Yes we have, and of the same description with that by which the movement of glaciers over rocks *in situ* is established. In August 1850, when Mons. Charles Martin and myself were in the west of Scotland, we visited Mr Smith of Jordanhill, then residing at Helensburgh; and that gentleman kindly showed us how and whence he had obtained many of the arctic shells, which have furnished, in his hands, so important a link in the chain of evidence proving the existence of a glacial climate in Scotland at the period of the boulder clay. He pointed out to us at the same time, on the beach at Row, a number of blocks embedded in the clay, with their upper surfaces striated; and called attention to the fact, that though these blocks were scattered irregularly over a considerable area, the striæ on all of them were parallel, or pointed in one direction, that is, N.N.W. and S.S.E. From this he inferred that they had been striated in the exact position which they now occupy, and have remained unmoved since the ice passed over them. In my "Geology of Fife and the Lothians," published in 1839, I expressed a similar opinion (page 213) as to the striated blocks embedded in the boulder clay near Edinburgh. The evidence, however, was not unequivocal, and I was led to doubt its soundness on reading Agassiz's *Etudes sur les Glaciers*, published in 1840, and felt then inclined to believe that the striæ were better accounted for by assuming that the blocks had been embedded in the bottom of a glacier; that during the downward motion of the icy mass, they had been striated themselves while cutting striæ on the rock below (a reciprocal action easily understood); and that they had been afterwards mingled with the boulder clay, when that deposit was formed (as then supposed) out of the wrecks of the moraines, by the fusion of the ice, or an irruption of the ocean. A month or two afterwards, I had an opportunity of testing Mr Smith's conclusions in the neighbouring locality of Gareloch, where rocks striated *in situ* are so abundant, and I found them fully confirmed. There were striated blocks there lying loose on the surface, from which nothing could be inferred; but every striated block *embedded in the clay* on the beach was striated in one invariable direction, which was precisely that of the valley, namely, N.N.W. and S.S.E. They were not numerous (I counted eight), but the parallelism of their striation was perfect, and I was surprised and mortified to find, that when I examined the district, and published my account of the striated *rocks* of Gareloch, three or four years previously (Edin. Phil. Journal, Jan. 1846 and Jan. 1847), the parallel striation of the *boulders* had been entirely overlooked. It was Mr Smith's observations at Row, that brought me back to the opinion which I had too hastily renounced. In 1852 or 1853, Mr Hugh Miller and Mr Robert Chambers discovered whole acres of blocks embedded in the clay between Leith and Joppa, all striated in one uniform direction (E.N.E. and W.S.W.), which agrees correctly with the striation of the fixed rocks in the neighbourhood.

If we infer the ancient existence of glaciers, and the direction in which they moved, from the parallel striæ on rocks *in situ*, we cannot refuse to receive the *parallel* striation of blocks embedded in a matrix of clay, as evidence of the same import. I have no doubt, then, that when the subsoil of the plain below Corusk is exposed, embedded blocks will be found in it, and striated in the direction of the valley, namely, N.W. by N., and S.E. by S. Upon these, as a floor or bottom, the glacier had moved, and the mounds D, E, F, are remnants of its terminal moraines, that is, of the clay, sand, gravel, and blocks, which being borne on its surface, were dropped over its lower end, and perhaps afterwards pushed before it. With regard to the mound C, its smooth sloping sides give it a different character, and render it probable, that it is merely a protuberant portion of the bottom. That bottom may consist of the old stiff boulder clay; but judging from what is seen on the banks of the river, it more resembles the upper and looser clay, and it may even be the lower portion of the matter constituting the moraines. The semi-fluid constitution of glaciers, as described and illustrated by Professor Forbes, leads to the conclusion, that under certain circumstances, they will travel over such a deposit. For, as the clay and gravel of moraines has more than twice the specific gravity of ice, it is evident that the power of a glacier to drive a large mass of such materials before it, must cease at a certain point, since beyond that point it will be easier for the glacier to dilate itself upward, than to overcome the resistance in front. But as it swells upward, the resistance in front will diminish, and the glacier may push the upper part of the moraine before it, while it glides over the lower; or it may override the whole, and the ultimate effect will then be to raise the glacier to a higher level. It is perhaps in this way that the insignificant size of the terminal moraines of some large glaciers (such as that of the Unter Aar) is to be accounted for. They may be, as it were, engulfed, in consequence of the glacier riding over and covering them.

It is not difficult to find a probable cause for the glacier of Glenmessan stopping at AB, and piling up its terminal moraines there. The lateral valley of Corusk *w*, though short is steep in the sides, and would have its separate glacier, whose course would be in the direction *k*, *b*, at right angles to the motion of Glenmessan glacier. When the latter, therefore, arrived at the line *g*, *h*, it probably encountered a rampart of ice flanked by piles of gravel and clay, issuing from the gorge at *k*, and stretching right across its path. Its southward march would thus be stopped, and stopped perhaps so long, that after the rampart of ice had disappeared, it was not able either to propel its own massive moraines A, B, or to override them. If the work of M. Collomb, *Preuves de l'existence d'Anciens Glaciers dans les Vallées des Vosges*, is consulted, it will be seen from his two maps (pp. 12 and 180), that the ancient moraines are generally found at the *junction of lateral valleys* with the principal one. Sometimes the glacier in the lateral valley had arrested the glacier in the principal, and sometimes the latter had arrested the former. In another point the ancient moraines in the Vosges illustrate those we have been describing; they are generally "multiple," consisting not of one mound or ridge, but of several, ranged, as he expresses it, *par echelon*, or one behind another. Occasionally there are two, like A, B—often there are three, and of various forms and magnitudes. At Kirchberg, for instance, there is a double one slightly curved, 400 metres long, and one of the mounds is 10 metres (=32½ feet) high. At Hussern there is one 15 metres (49 feet) high, at Sondernach one 50 metres (164 feet) high; at Wessenberg there is a triple moraine, the highest point of which is 35 metres (115 feet) above the river, which has cut a breach through the middle of the three ridges.

These ancient moraines are in general of a slightly curved form, with the concave side facing the upper part of the valley, but some are straight, as that of Hussern.

The mounds E, F, D are perhaps the work of the glacier of Corusk valley, but they may have been formed by that of Glenmessan before the mounds A B existed. At *n*, right in front of the opening of Corusk valley, there is a vertical precipice, 100 feet high, facing that opening, and as smooth as a wall of dressed masonry, though cut across the planes of the mica slate. Might we suppose that the glacier issuing from Corusk valley abutted against the rock here, cut it down vertically, and smoothed it?

The valley of Glen Eck, which unites with Glenmessan at *m*, contains many traces of glacial action. Abraded and rounded rocks are numerous on the east side of Loch Eck, and there are fine examples of striæ and groovings from the level of the water up to an elevation of 100 feet. They may be seen at various points within three miles from the foot of the loch, sometimes on highly inclined surfaces, and always running horizontally, or nearly so, leaving no doubt that a glacier many hundred feet in depth had moved along the valley. The loch is shut in at the foot by a flat, uneven mound of earth rising 25 feet above its surface, and through which the water has cut a winding passage. This mound is in reality the commencement of a plain that extends two miles southward, but it has much the aspect of a terminal moraine; and when we consider its size, materials, and position, as a barrier of clay and gravel shutting in a narrow cavity ten miles long, and rising 360 feet above the bottom of that cavity (for such is the depth of Loch Eck), we are tempted to think that it may be what its appearance indicates. If it ever was a moraine, the rude stratification seen on the banks of the stream shows that the materials composing its upper part must have been re-arranged, and of course under water.

We have seen that blocks embedded in the old boulder clay have their upper surfaces striated *in situ*. The question, then, presents itself, Do these striated blocks (which in some cases lie very compact, and, as it were, in one plane) only occur at one level, namely, at the top of the *old*, and immediately below the *newer* boulder clay, or do they occur at more than one level, as at the top, the middle, and towards the bottom of the older deposit? The conditions necessary to the formation of glaciers, and to that of the boulder clay, are apparently so different, that we can scarcely suppose them to alternate. The few facts known to me, however, rather favour the idea that they did alternate, or at least that the striated blocks occur at more than one level; but information is yet wanted on the subject.

Mr Robert Chambers, in a paper published two years ago, expressed an opinion that the mounds A B were probably parts of a *lateral* moraine of the glacier of Corusk valley. I consider this opinion altogether untenable; but as I learn from him that he has subsequently renounced it, nothing more need be said upon the subject.

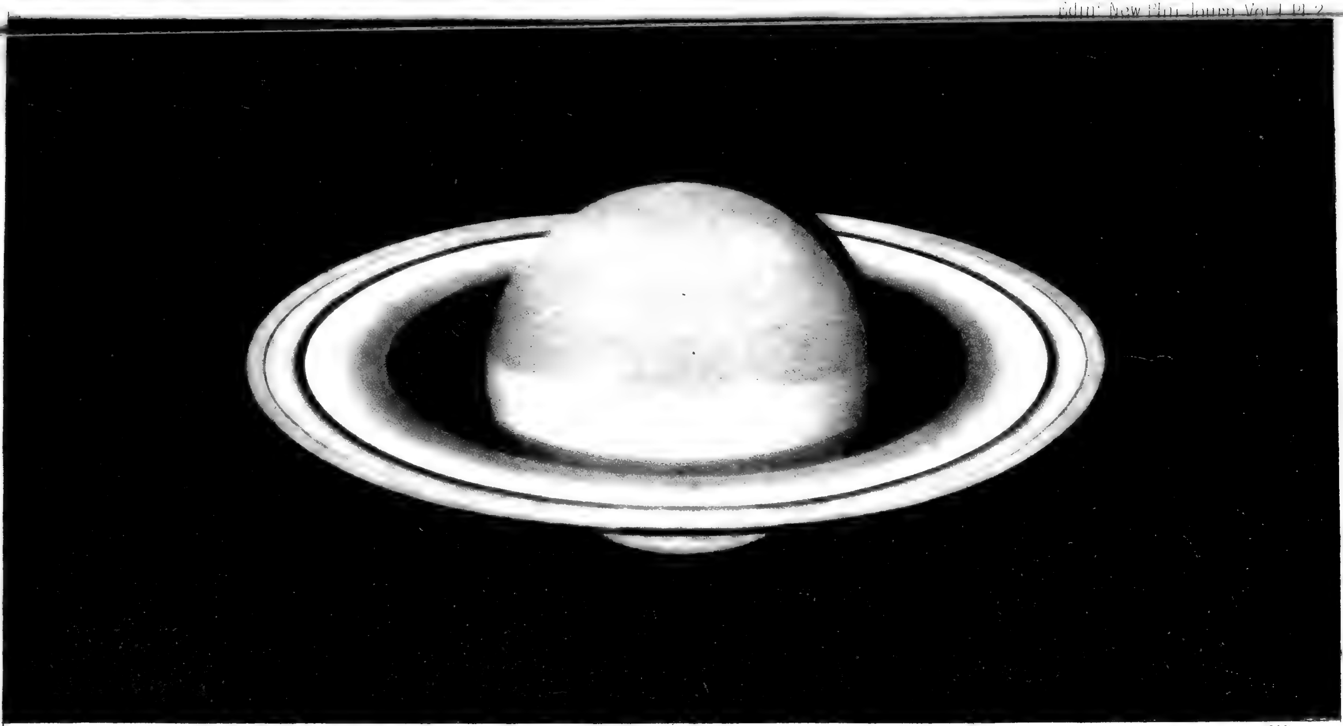
Note.—The description of the moraines in Glenmessan is the substance of a communication made *viva voce* to the Geological Section of the British Association, at the meeting in Edinburgh in 1850. But the discussion commencing at the foot of page 198 is an addition.

Physical Features of Saturn and Mars, as noted at the Madras Observatory. By Captain W. S. Jacob, H.E.I.C. Astronomer. (With Two Plates.)

Saturn.

Our knowledge of the physical features of the planet Saturn has received several important additions within the last few years; an eighth satellite discovered almost simultaneously in America and England, by Bond and Lassell,—the inner obscure ring, also seen about the same time by Dawes and Bond,—and the fine line or division in the outer bright ring; these are the most notable points that have been brought to light. The last two are not, indeed, strictly speaking, recent discoveries, since the obscure ring would appear to have been seen, and even some measurements of it made, by Dr Galle of Berlin in 1838, while several observers have at different times seen, or imagined they saw, one or more lines or markings on the outer ring. A notice by Captain Kater of such appearance, accompanied by drawings, is to be found in vol. iv. of the Memoirs of the Royal Astronomical Society. But these observations or discoveries do not seem to have gained much attention or credit at the time, or to have been followed up in any way, and the memory of them had almost died away, until revived by their re-discovery in 1850, since which time they have been verified by a host of vigilant observers, with powerful instruments; and some of them have recorded their experience in the form of drawings or engravings, so as not only to give the general public a pretty correct notion of what had been seen with the best instruments, accessible only to few, but also to preclude the possibility of the subject again sinking into oblivion.

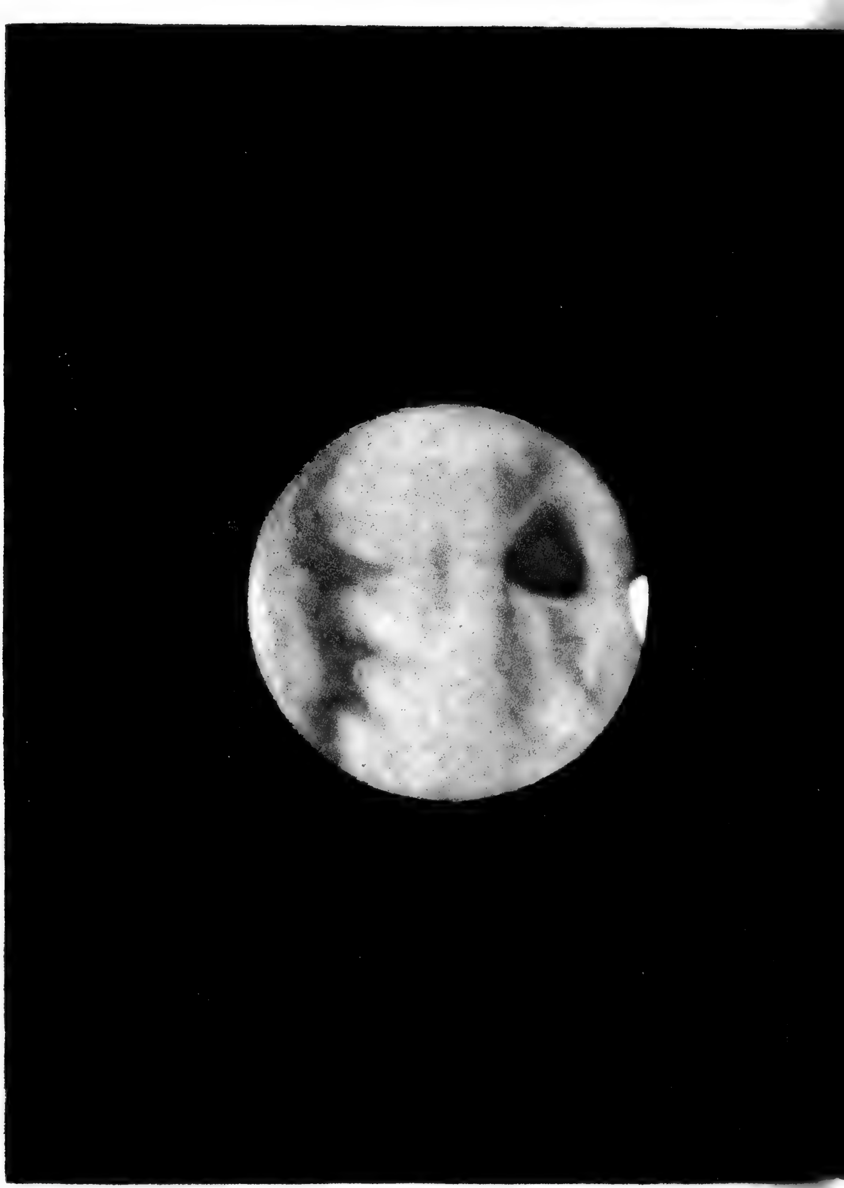
The plate here given (Plate II.) represents the planet as seen at Madras in the latter part of 1852, with the equatorial instrument constructed by Messrs Lerebours and Secretan of Paris, the object glass of which has an aperture of $6\frac{1}{4}$ inches, and a focal length of 88·6 inches, and whose defining power is of a high order. Other favourable circumstances were, the planet's proximity to the zenith, and the tranquillity and transparency of the atmosphere. The obscure ring was well brought out the first time it was looked for, and the fine line on the outer ring was also seen distinctly enough to allow of good measures being made with the filar micrometer, although, strange to say, its very existence is still questioned in some quarters, as it is not visible in some of the largest telescopes, such as the Poulkova Refractor; very neat definition, rather than a great amount of light, being required for the purpose. The transparency of the obscure ring, exemplified by the planet's limb appearing through it, would seem to have been first noticed at Madras, being shown in a drawing taken on 22d September 1852, and forwarded to a friend in this country, in a letter dated 11th October. This ring, as seen across the planet, has a light umber-brown tint, and a filmy, smoky character; the division between the two principal rings (usually represented black) had nearly the same tint, while its outer edge was not sharply defined, but shaded off, as shown in the engraving. No separation, either by a dark or bright line, could be discerned between the bright and obscure rings; on the contrary, the impression was that the shading in the former was produced by the latter overlapping or enveloping its edge.

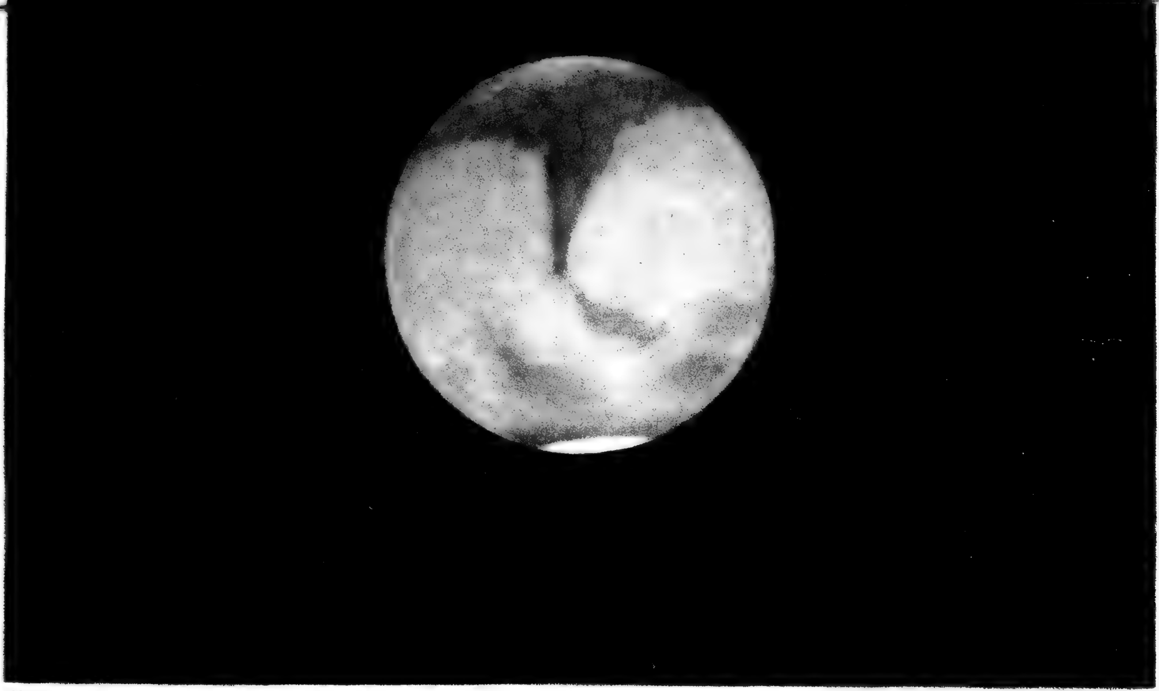


W Forrest sc.

SATURN, ON THE 15TH OF NOVEMBER 1852,

From the drawings of W. S. Jacob, Esq. H. E. I. C. Astronomer.





W. J. Jacob del.

W. Forrest sc.

MARS, AS SEEN AT MADRAS IN THE LEREBOURS EQUATORIAL

The upper figure represents the Planet as seen on 18th March 1854 at 9h 30m Madras mean time and the lower on 23d March at 6h 54m, the two differing by about 90° of longitude; the other faces of the Planet present nothing very remarkable.

The planet was frequently examined, whenever the atmosphere was in a favourable condition, until April 1854, the time of the writer's departure from India, without any change being perceptible, except that the peculiar features above described had become gradually rather more conspicuous, so as to be discerned with lower powers. This would arise partly from the rings appearing at a greater inclination, or more *open*, and partly, perhaps, from the eye becoming, through practice, more familiar with the details. After the first scrutiny, in August 1852, no difficulty was ever experienced in making out any of the peculiar points above described, provided that the atmosphere was sufficiently tranquil to admit of using a magnifying power of 180 or upwards. The powers usually employed were 277 and 365.

Mars.

The views of Mars (Plate III.) were taken with the same instrument. The lower view, though the later in point of time, yet *precedes* the upper, as regards the longitude or angular motion of the planet, because its period of revolution is rather longer than that of the earth; the difference in longitude between the two is about 90°. The other faces do not present such striking features, but are nearly blank. Former engravings of the planet do not show any such distinct markings; at least the writer has not been fortunate enough to meet with any that could be recognized as likenesses. Mars will not again be in a favourable position for observation until 1856; in 1858 he will be nearer still; and it is to be hoped that on these occasions still better drawings of him will be obtained.

W. S. Jacob.

21*st* *Nov.* 1854.

A recent Revision of a Portion of the Catalogue of Stars published by the British Association in 1845. By Captain W. S. Jacob, H.E.I.C. Astronomer at Madras. Communicated by Professor C. Piazzi Smyth.

Notes by Professor C. Piazzi Smyth *at the time of communication of this Paper to the Royal Society.*

This paper is short, but important; yet, though important, it can hardly but be somewhat dry and uninteresting to those not immediately engaged in the pursuit of exact astronomy, and not conversant with the foundations on which it rests.

With our earth turning on its axis, and revolving about the Sun in an orbit continually changing in every element, and with the Sun itself describing a similar orbit about some other sun or suns, there may well be difficulty in finding any truly fixed and immoveable objects from which our measurements of the moving ones may be reckoned.

The so-called fixed stars are not fixed, and the nearer are sensibly displaced by the amount of the Sun's movement, as well as by having proper motions of their own. Hence, though the larger stars are a very convenient system of milestones whereupon to begin our measurement of celestial arcs, yet they are not to be implicitly depended on. They can only be looked on as intermediate, and must have their reputed fixity tested by comparison with the more distant stars, and especially with the mean of an immense multitude of them, whose varying aberrations may, on the whole, tend to balance each other, and to exhibit a constancy of which no single star is capable.

To this end, accordingly, the efforts of most of our public and many of our private astronomers have long been directed; and an exceedingly important step was taken by the British Association a few years ago, in the publication of their large Catalogue of 8377 stars. Some persons, indeed, were inclined to think it rather premature, as many of the stars rested on old and rather scanty and apocryphal observations; but others contended that the publication of a catalogue so made up, and duly pointing out the good and the bad material, would incite astronomers to additional exertions in perfecting all that was possible.

This argument fortunately prevailed; and the present paper is one of its expected fruits.

On the British Association Catalogue of Stars.

The Catalogue of Stars published by direction of this Association in 1845, has long established its place as a valuable work of reference, and it is therefore highly important that any errors which it may contain should be made known and corrected.

It contains the places of 8377 stars, brought up to 1850, from the best data available at the time; and all possible care appears to have been used in collating the different authorities, so as to obviate error. The great majority of places may, therefore, be considered as very exact; but to those of a considerable number, especially of the southern stars, there was some doubt attaching, because of their dependence on the determinations of Lacaille or Brisbane, neither of which, from the imperfection of the means employed, can be considered as coming up to the standard of accuracy expected at the present day.

A thorough revision of these was therefore obviously desirable, and I had planned such a revision some time before my arrival at Madras in July 1849, and commenced it within the month of my taking charge of the Observatory. There was a manifest propriety in the selection of Madras as the place of revision, inasmuch as Taylor's observations at that observatory had been made the original ground-work of the Catalogue.

My plan was to determine, by at least three observations with each instrument (5-feet transit and 4-feet mural circle), the place of every number between north polar distance 40° and 155° to which the slightest doubt attached; in fact, all those within that range which had not been observed by Taylor. The north circumpolar stars I considered would be better fixed in Europe, and Mr Johnson, I knew, had taken them in hand. A few stars were, however, observed beyond the limits above-mentioned, especially to the north, when there happened to be leisure for them; but they were not specially sought after.

The observations were very nearly completed in about three years, or before the end of 1852; but a few numbers, that from various causes had been missed, were observed in the spring of 1853. The reductions occupied about one year, and were completed by the middle of 1853. The results have been printed in the last volume of Madras Observations, which should now be on its way to the India House. The volume was not quite completed at the time of my departure from India, but a few extra copies of the Catalogue were struck off, and have been for some time in the hands of several of the Fellows of the Royal Astronomical Society.

The results of the revision may be summed up as follows:— In all, 1503 numbers were examined; of these the under-mentioned 55 were missing, viz.:—

186
278
434
534
601 dup. of 596
642
931
935
969
2018
2686
3233
3328 dup. of 3323, 1^{m}
3401
3454
3461
3482
3535
3586

3707 dup. of 3706, $5'$
4399
4569
4983
5025
5162
5241 dup. of 5247, 30^{s}
5349 ... 5350, 10^{s}
5415
5482
5491
5524
5662
5665
5672
5685
5707
5725
5738

5741
5770 dup. of 5772
5816 ... 5815
5849
5923
5928
6542
6725
6770
6775
6898
6917
7203 dup. of 7210, 1^{m}
7214 ... 7225, 1^{m}
7467 ... 7466
7576 ... 7575, 2°
8042

Of the above, six are accounted for by being duplicates of numbers representing real stars, whose right ascension has been erroneously recorded by 1^{m}, 30^{s}, or 10^{s}; errors which *will* sometimes occur in single determinations; two more appear to be duplicates, with errors in polar distance of $5'$ and 2^{s} respectively; and three more are probably duplicates, but with small uncertain errors.

Twelve numbers marked as nebulæ were examined, viz.:—

2511
2766
3247
3547
3692
3944
4485
5040
5300
5470
6201
7457

and were found to be clusters of small stars, more or less dense. Of these, five contained a star sufficiently conspicuous to be identified by the two instruments, but the places of the remaining seven could not be accurately fixed.

The places of 1440 numbers were recorded. Of these, by far the greater part were observed four times or upwards, and only a very few less than three times; which arose either from the object being too faint for our instruments, unless under unusually favourable atmospheric conditions, or else from the numbers following so thickly in right ascension that they could not all be observed so often without waiting for another year.

The following thirteen numbers have companions, whose places have been entered in the Catalogue, viz.:—

776
1728
1752
2511
3067
3118
4513
4558
5111
5673
6579
6984
7963

Eighteen more have also companions, more or less distant, whose places have been approximately fixed, and set down in the notes accompanying the Catalogue, viz.:—

13
450
936
1712
1752
1999
2687
2738
6132
6163
7327
7417
7483
7631
7810 (H. and S. 343)
8101
8253
8272

Four more are noted as double, but without fixing the companions, viz.:—

2688 which is H. and S. 88.
4573
6835
7699

The following 71 numbers are found to differ from the British Association Catalogue by more than 2^s in right ascension, or 10″ in polar distance, viz.:—

15
157
193
602
728
1412
1790
2048
2121
2190
2284
2610
3008
3067
3139
3189
3567
3639
3659
3694
4041
4512
4519
4912
4968
4979
5114
5117
5288
5372
5389
5459
5540
5564
5570
5612
5722
5879
5897
5898
5916
5977
6000
6011
6032
6165
6173
6185
6212
6219
6303
6374
6578
6818
6928
7017
7055
7163
7180
7268
7307
7347
7594
7631
7699
7769
8011
8164
8260
8278
8306

Of these, 17 are accounted for as mistakes of gross quantities, such as 1^m, 30^s, or 10^s of right ascension; and 10′, 5′, or 1′ of polar distance. Several others may possibly be corrected by a reference to Lacaille's original observations, and some of them are probably cases of proper motion. Besides these, there are 29 stars from Brisbane's Catalogue, in which the difference of right ascension is between 1^s and 2^s; some of these also may turn out to be cases of proper motion.

With the above exceptions, the agreement of the observed places with the Catalogue is in general very close. In the cases of those stars whose places depend upon Groombridge remarkably so; a difference of $0^s{\cdot}2$ in right ascension, or of 2″ in polar distance being comparatively rare; so much so as to render it probable that anything much exceeding that amount must arise from proper motion. With regard to this point, the proper motions assigned in the Catalogue have been in a few cases confirmed, but in those of the greater number of southern stars, they have been either negatived or rendered doubtful; these will, therefore, have to be observed after the lapse of a few years, in order to set the question at rest.

I should state that only a small proportion of the observations were made by myself, the great mass being taken by the native assistants, and the work may be considered as creditable to them. The computations were either made in duplicate and compared, or were made by one party, and the results examined in a different manner by another; and nearly the whole of them underwent a thorough revision by myself. The amount of labour thus involved in the reduction of nearly 12,000 observations will not easily be conceived, except by those who have been accustomed to operations of the like kind.

At the time of undertaking the above work, I was not aware that my friend Mr Maclear was about the same time commencing a similar revision at the Cape Observatory. His plan of proceeding, however, being somewhat different from mine, our results, while partly confirmatory, will be in a great measure supplementary to each other, especially as he would be able to fill up the circle of 25° round the south pole, which was beyond my range. As the northern circumpolar stars have been carefully gone over at Oxford, the revision of the Catalogue may now be considered as pretty complete.

W. S. Jacob.

This state of things described by the author is, therefore, highly encouraging, and we seem now on the point of possessing ample materials for the construction of a far finer catalogue of stars than the world has yet seen.

The notice of the indications of interesting discoveries of proper motions of stars, as given by the Australian observations of our venerable President (Sir Thomas M. Brisbane), will be read with much pleasure; while the mention of the accuracy of Groombridge's Catalogue will be extensively received as another illustration of the never-dying character of good astronomical work. Mr Groombridge was past fifty before he had leisure and means to apply himself to astronomical observation. When he did begin, he worked zealously and well; and procured, before many years were over, above 50,000 observations. But the computation of these was a far longer work: he laboured at it and died; his wife, the partner of his domestic cares and astronomical anxieties, died also; they were followed by his friend and adviser, the then Astronomer Royal; by his friend and helper, the then Superintendent of the Nautical Almanac; and by his clerk and assistant. These all died before the Catalogue, passing into other hands, received its final correction, and was published to the world, wherein it has now been found to be so excellent and valuable a contribution to astronomy.

One more remark presses itself upon me. The extinction of one star, 3000 years ago, led Hipparchus to the formation of the first catalogue of stars. Captain Jacob now reports 55 as missing. Are not, then, the phenomena of our day as important as those of any other, and should they not equally induce to the study of astronomy? C. P. S.

Some Additional Experiments on the Ethers and Amides of Meconic and Comenic Acids. By Henry How, Professor of Natural History, King's College, Windsor, Nova Scotia.

The following pages contain an account of some researches made for the purpose of rendering more complete the papers I have already published on meconic and comenic acids. They relate, in the first place, to the confirmation of some facts formerly announced, but which have been since disputed; and secondly, to descriptions of new methods for producing certain compounds already described, and finally indicate the existence of some entirely new substances, derived from the acid of opium.

In my investigation of meconic acid,* I described an acid amide, which I termed meconamidic acid, and assigned to it a complicated formula, having no analogy with that of any known body. The want of simplicity and of this analogy in my expression, has led to its being objected to by Messrs Wurtz † and Gerhardt,‡ whose critical opinions are entitled

---

* Trans. Roy. Soc. Edin. xx., Part iii.

† Ann. Ch. Phys. 38, 195.

‡ Chimie Organique.

to such weight as to compel me to return to the subject, and endeavour either to discover the source of error, or to show sufficient reasons for the validity of my own assumption. The substance alluded to was produced by the action of ammonia on the monoethylated meconic acid; a yellow salt being thus formed having a most peculiar non-crystalline structure, as it occurs in the state of transparent rounded grains, which completely resemble drops of oil. This is an ammonia salt, from which an excess of hydrochloric acid throws down the acid in question as a white crystalline powder or crust.

I now recapitulate the analyses of this substance, as formerly published, and add some remarks from my note-book, made at the time of the analyses, but not given with them in the paper. I do this in order to meet the objections taken to the conclusions drawn from these experiments, on the ground that the substance possibly contained ammonia, and, not being crystallizable, might not easily be obtained pure. The analytical numbers I place in juxtaposition with my own formula, and that proposed by Wurtz and Gerhardt, which is indeed that of the substance I was in search of, and which, after a similar comparison in the paper before mentioned, I was compelled to reject as discordant with experimental evidence. The figures are:—

	I.	II.	III.	IV.
Carbon, .	39·73	39·65	39·50	
Hydrogen,	3·30	3·32	3·26	
Oxygen, .	49·13	48·98		
Nitrogen, .	7·84	8·05		7·70
	100·00	100·00	100·00	100·00

and I find, by referring to my notes, that I. and II. were from materials of different preparations, in each case consisting of solution of the yellow granular salt in hot water, addition of strong hydrochloric acid in excess and the subsequent recrystallization from boiling water of the substance so precipitated. The remark noted after these results is, "ammonia seems to adhere to this substance," and the residue of that employed for analysis II. was dissolved in boiling water, some strong hydrochloric acid was added, and the heat kept up for some minutes; this process furnished the acid whose analysis (III.) clearly shows it to remain quite unaltered.

Since the publication of the paper, I have again taken some of the yellow salt, and boiled its aqueous solution till no trace of ammonia was perceptible, and its colour was quite discharged; muriatic acid added to it while still hot threw down a substance furnishing these results:—

4·602 grains, dried at 212°, gave
6·770 ,, carbonic acid, and
1·310 ,, water;

leading to a percentage of

Carbon, 40·12
Hydrogen, 3·16

agreeing perfectly with the former numbers and those required by the calculation. The mean of all these analyses is collated anew with the two formulæ, and their values:—

	Mean Expt.	H. H.			W. and G.		
Carbon,	39·75	39·84	C_{84}	504	42·21	C_{14}	84
Hydrogen,	3·26	3·08	H_{39}	39	2·51	H_5	5
Oxygen,	49·13	49·34	O_{78}	624	48·25	O_{12}	96
Nitrogen,	7·86	7·74	N_7	98	7·08	N	14
	100·00	100·00		1265	100·00		199

The empirical formula I assign to the meconamidic acid being

$$C_{84}\,H_{39}\,O_{78}\,N_7\,;$$

and that of the proposed normal amido-meconic acid,

$$C_{14}\,H_5\,O_{12}\,N.$$

With regard to the basicity of my compound, I stated before that I had only one salt from which to draw conclusions, and I must now mention a fact which induces me to assign to it another as its normal saturating power. In the description of the yellow salt already so often adverted to, I showed that it loses ammonia both in boiling water and when dry, at 212° Fahr. And I now find that its aqueous solution evaporated on the water bath to complete dryness, leaves a crystalline mass of colourless silky needles. An accident deprived me of the material destined for a combustion, but the amount of nitrogen was determined by the following experiment:—

4·495 grains, dried at 212°, gave by Péligott's method
0·645 „ nitrogen = 14·34 per cent.

and agrees neither with that required by a neutral nor an acid salt of amido-meconic acid, whose values and formulæ are:—

	Acid Salt.			Neutral Salt.		
Carbon, .	38·88	C_{14}	84	36.05	C_{14}	84
Hydrogen, .	3·70	H_8	8	4·75	H_{11}	11
Oxygen, .	44·46	O_{12}	96	41·18	O_{12}	96
Nitrogen, .	12·96	N_2	28	18·02	N_3	42
	100·00		216	100·00		233

but rather with a salt of meconamidic acid, differing from the yellow one in being anhydrous, and containing three atoms less of ammonia. The formula—

$$6\ NH_4O,\ C_{84}\ H_{24}\ N_7\ O_{63}$$

requires 14·15 per cent. nitrogen; the above result being 14·34. A glance at the rational formulæ I assigned to the yellow salt and to the acid itself on the former occasion of their description,

Salt, $9\ NH_4O,\ C_{84}\ H_{24}\ N_7\ O_{63} + 3\ aq.$
Acid, $9\ HO,\ C_{84}\ H_{24}\ N_7\ O_{63} + 6\ aq.$

will show that these cease to be tenable when the existence of the new salt, and its mode of formation, are taken into account. It seems to me that the facility with which the yellow compound loses its ammonia should cause it to be considered rather a super-salt or basic form of combination, and that the stability of the white crystalline one affords more correct data by which to determine the saturating power of the acid; and I would now represent the three thus:—

Meconamidic acid, $6\,HO, C_{84}\,H_{24}\,N_7\,O_{63} + 9HO$.
Yellow ammonia salt, $6\,NH_4O, C_{84}\,H_{24}\,N_7\,O_{63} + 3\,NH_3 + 6\,HO$.
White „ „ $6\,NH_4O, C_{84}\,H_{24}\,N_7\,O_{63}$.

In the present state of our experimental evidence on these points, therefore, I must conclude this part of the subject with expressing my conviction that the meconamidic acid, having at least the empirical constitution assigned to it by myself, is a stable substance, and capable of entering into two distinct combinations with ammonia, of widely different appearance and characters. If this be true, the cause of the want of analogy will certainly be discovered at some time, and, if not, my errors may enable some one else to light upon the normal amido-meconic acid I sought to obtain.

Action of Ammonia on Biethylated Meconic Acid.

Biamido-Meconic Acid.—When the second normal ether of meconic acid is boiled for some time with an excess of ammonia, it is converted into an amidogen acid, which is thrown down from the cooled liquid as an amorphous powder; when redissolved in boiling water, the new substance again separates, in the cold, in an amorphous state:

5·440 grains, dried at 212°, gave
8·440 „ carbonic acid, and
1·590 „ water.
6·130 „ dried at 212°, gave, with soda-lime,
13·350 „ platinum salt of ammonia.

	Expt.	Calc.		
Carbon, .	42·31	42·42	C_{14}	84
Hydrogen, .	3·24	3·03	H_6	6
Oxygen, .		40·41	O_{10}	80
Nitrogen, .	13·67	14·41	N_2	28
	100·00	100·00		198

The rational formula of this body, considered as a monobasic acid, as its origin would indicate, is

$$HO, C_{14}\,H_5\,N_2\,O_9,$$

in the air-dry state it has an additional equivalent of water, with which it readily parts, as is shown by this experiment.

$$\begin{cases} 14{\cdot}710 \text{ grains, air dry, lost, at } 212^\circ \\ 0{\cdot}535 \quad \text{,,} \quad \text{water,} \end{cases}$$

equal to 4·31 per cent., and 4·45 is required by the loss of one atom of water in the formula

$$HO, C_{14} H_5 N_2 O_9 + aq.$$

The amide is clearly derived from the ether acid by the change of two equivalents of alcohol for two of ammonia—

$$\underbrace{HO, 2\,C_4 H_5 O, C_{14} HO_{11}}_{\text{Biethylomeconic acid.}} + 2\,NH_3 =$$

$$2\,(C_4 H_6 O_2) + \underbrace{HO, 2NH_2, C_{14} HO_9}_{\text{Biamido-meconic acid.}}$$

and its relation to bibasic meconate of ammonia is—

$$HO, 2\,NH_4O, C_{14} HO_{11} - 4\,HO = HO, C_{14} H_5 N_2 O_9.$$

Biamido-meconic acid has not been observed in the crystalline state; to the naked eye it presents the appearance, in the present form in which I have obtained it, of a grayish-white amorphous powder; it is difficultly soluble in cold water and in dilute acids, and is readily decomposed when heated with the fixed caustic alkalies. It reacts strongly acid, and decomposes with effervescence the carbonated earths when its solution is heated with them, and forms, like the other acids of this group, basic combinations when an excess of the earthy constituent is employed. Although from its derivation there can be little doubt that it is really a monobasic acid, I have not been able, with the amount of material in my possession, to confirm this assumption, my substance being exhausted in fruitless efforts to obtain neutral salts. By solution of the acid in an excess of ammonia and subsequent evaporation to dryness at 212°, a salt of difficult solubility, even in hot water, is obtained, whose solution gave with chlorides of calcium and barium amorphous precipitates in which the amount of base was greatly above that of neutral salts, and yet not enough to correspond with any other atomic expression.

Triethylated Meconic Acid?—In my former paper on meconic acid, I shewed that when it is distilled with absolute alcohol and a small quantity of oil of vitriol, the biethylated meconic acid is produced. If the proportion of sulphuric acid be much increased in this process, none of the second ether is obtained; dilution of the contents of the retort throwing down a black oily mass of very different characters. It is excessively soluble in spirit, and is again thrown down as black oil by water. I did not analyse this substance, but think it may possibly be the triethylated meconic acid.

$$3\,C_4\,H_5\,O, C_{14}\,HO_{11}.$$

Digestion with ammonia at a gentle heat converts it into a blackish brown powder—the corresponding amide?

Action of Iodide of Ethyl on Comenic Acid.

Ethylocomenic (*Comenovinic*) *Acid.*—When comenic acid in fine powder is heated with spirit of wine and iodide of ethyl in a sealed tube, at 212°, it dissolves, though very slowly, and a fortnight's continued application of this heat is required to effect the solution of a few grains. The product of the reaction consists of two substances; and as I found the same result, to all appearance, to be brought about more speedily at a higher temperature, I at once resorted to this method.

About 70 grains of the powdered acid were heated with a few fluid drachms of rectified spirit and a little iodide of ethyl, in a close tube, to nearly 350° Fahr.; solution was complete in two hours. The vessel gave no deposit upon being then allowed to stand cold for twenty-four hours, but directly it was cut open, a slight explosion occurred, and crystals began to form, increasing so rapidly in quantity as speedily to render the whole interior solid. These crystals were in the shape of prismatic needles, with minute, rounded, opaque grains among and upon them, here and there throughout their mass. They were thrown upon a filter, and their mother liquor, which was very dark-coloured, and contained much hydriodic acid, was allowed to drain off. After being washed with cold water, they were dissolved in boiling water, and the first crop of crystals from the still warm fluid, which consisted merely of the needles, without any of the grains, was collected, and washed with tepid water. Upon being dried they furnished these results:

4·903 grains, dried at 212°, gave
9·300 ... carbonic acid, and
1·935 ... water.

	Experiment.	Calculation.		
Carbon, ...	51·73	52·17	C_{16}	96
Hydrogen, . .	4·38	4·35	H_8	8
Oxygen, ...	...	43·48	O_{10}	80
	100·00	100·00		184

which agree perfectly, as the above comparison shows, with those required by the comenovinic acid I formerly described,

$$HO, C_4 H_5 O, C_{12} H_2 O_8.$$

That it is one of the two basic atoms of water of comenic acid which is here replaced by ether is proved by the deportment of the new substance with ammonia. It dissolves readily in an alcoholic solution of this alkali, forming the beautiful yellow crystalline salt I before showed to be produced by the union of comenic ether and ammonia. It was mentioned at the time, as a characteristic of this salt, that it loses all its base at 212°. This I find to be the case with that from the new compound; I conclude, therefore, that this is the true comenic ether, for I conceive that had the action of $C_4 H_5 I$ on comenic acid, consisted in the formation of a bibasic acid, in which one atom of hydrogen is replaced by ethyl,

$$\left.\begin{matrix} HO \\ HO \end{matrix}\right\} C_{12} H (C_4 H_5) O_8$$

analogous in derivation to the methylosalycic acid of Cahours and Gerhardt, it would have possessed very different powers of combination with bases from that of the feeble comenic ether.

I attempted to procure an amyl compound in the same way; comenic acid, chloride of amyl, and spirit of wine, being heated in the oil-bath, at 300° Fahr., till solution was complete, which took place in twelve hours. Somewhat to my surprise, the reaction proved to be identical in products to the former; from a liquid containing hydrochloric acid a deposit of needles and grains fell. The needles, when separated as before, gave these numbers:—

$$\left\{\begin{array}{rll} 5{\cdot}667 & \text{grains,} & \text{dried at } 212^\circ\text{, gave} \\ 10{\cdot}720 & \ldots & \text{carbonic acid, and} \\ 2{\cdot}400 & \ldots & \text{water.} \end{array}\right.$$

equal to a percentage of

Carbon, . .	51·60
Hydrogen, . .	4·90

which approach so nearly to the last results, as to admit of no doubt that they relate to the same substance, though not so pure as was before analysed; the reaction, I apprehend, being

$$\left.\begin{array}{l}HO\\HO\end{array}\right\} C_{12}\,H_2\,O_8 + C_{10}\,H_{11}\,Cl + C_4\,H_6\,O_2 = \left.\begin{array}{l}HO\\C_4\,H_5\,O\end{array}\right\}$$
$$C_{12}\,H_2\,O_8 + C_{10}\,H_{12}\,O_2 + H\,Cl.$$

Meconic acid, as might have been anticipated, is found to behave in the same manner as comenic acid, with iodide of ethyl. At 212°, in four or five hours it is converted into the same substances, carbonic acid being produced, which causes the tube to open with a lively explosion on its being cut with a file.

The grains alluded to above were never obtained in sufficient quantity, or in a state pure enough for analysis, but the tendency they had to reduce the percentage of carbon and hydrogen was remarked in some unsuccessful analyses of the needles, not quoted on that account. They consisted of an acid substance which dissolved in boiling water with great ease, and came down upon cooling in the same peculiar granular form. This character separates them from comenic acid, which I at first supposed them to be, and reminds one of the paracomenic acid of Stenhouse, which has, according to this chemist, the same composition. The formation of the para-acid, in this case, would be quite analogous to that of the change undergone by malic acid when heated with water.

I attempted to ascertain whether comenic acid really undergoes this alteration in a heated sealed tube with water. I find that under these circumstances, it is readily altered, and the first change seems to be the increased solubility of the acid. In four or five hours, at 300° Fahr., a great deal of the acid goes up in the water, which acquires a high colour, and, if then allowed to cool, deposits a granular matter. If the heat be continued, the contents of the vessel become in a few days a shining black solid, and complete decomposition seems to have taken place, for carbonic acid is produced in abundance. I have not been able to prosecute these experiments, which, with modifications, might lead to interesting results.

Action of Hydrochloric acid gas upon Comenamic Acid in Alcohol.

Hydrochlorate of Comenamic Ether (*Comenamethane*).—When comenamic acid ($HO, C_{12}H_4NO_7$) is suspended in absolute alcohol, or very strong spirit, through which dry hydrochloric acid gas is passed, the greater part of the solid dissolves, and remains in solution when the liquid has been cold for some time. The clear fluid leaves, on evaporation to dryness at 212°, an oily mass which dries up, by constant stirring at this heat, to a white or gray solid. If water be poured upon this residue after it has been heated for some hours, it dissolves a certain quantity with the production of a very acid liquid, while a considerable quantity of pure comenamic acid remains behind; the solution contains much hydrochloric acid, and if suffered to stand, deposits more comenamic acid as a crystalline powder, and, under some circumstances, long needles. If alcohol be employed instead of water, and it be added as soon as the mass is quite dry and has cooled, the whole dissolves readily, and by very cautious proceedings, a curious compound may be obtained, which is definite, though instable, and proves to be a combination, in fixed proportions, of comenamic ether and hydrochloric acid. The material used in the following analyses was procured by allowing an alcoholic or ethereal solution of the fresh residue above described to evaporate spontaneously; it was dried for analysis *in vacuo*, with oil of vitriol and solid potass in the receiver. The chlorine was determined by direct precipitation with nitrate of silver.

I. 4·175 grains, dried *in vacuo*, gave
6·182 ... carbonic acid, and
1·960 ... water.

II. 5·172 ... dried *in vacuo*, gave
7·674 ... carbonic acid, and
2·240 ... water.

4·22 grains, dried *in vacuo*, gave with soda lime,
4·09 ... platinum salt of ammonia.

3·147 ... dried *in vacuo*, gave
1·872 ... chloride of silver.

	Experiment.		Calculation.		
	I.	II.			
Carbon,	40·38	40·46	40·42	C_{16}	96
Hydrogen,	5·21	4·81	5·05	H_{12}	12
Oxygen,	...	...	33·70	O_{10}	80
Nitrogen,	...	6·08	5·89	N	14
Chlorine,	14·72	...	14·94	Cl	35·5
	100·00	100·00	100·00		237·5

the rational expression of the above resolves itself, for sufficient reasons, into this formula :—

$$C_4 H_5 O, C_{12} H_4 NO_7, 2 HO + HCl.$$

for I shall presently shew, that the hydrated comenamic ether is readily obtained from this substance in a pure state, by removing the hydrochloric acid with which it is here combined.

The hydrochlorate of comenamic ether dissolves with ease in warm water, and the solution deposits first comenamic acid, and finally, after long standing, some needles, which are possibly the ether itself. It dissolves to a large extent in cold alcohol and ether, and these fluids leave it by spontaneous evaporation, as a finely crystallized mass of long radiated silky needles. Alkalies also readily take it up, chloride of the base and the simple ether at once resulting from their contact.

Hydriodate of Comenamic Ether.—I have obtained indications of the existence of a compound similar to the preceding, with hydriodic acid. It was obtained by the action of iodide of ethyl on comenamic acid in absolute alcohol ; the reaction took place in a sealed tube in the oil-bath, heated for some time to about 300° F. When cold, the contents of the vessel separated into two liquids, one colourless, or yellowish, floating on the surface of a deep brown thick fluid. The upper stratum proved to be pure sulphuric ether, the lower one contained much hydriodic acid, and when evaporated to dryness at 212°, left a gummy residue, which solidified into a mass of yellow striated needles. After these had been dried *in vacuo*, they afforded evidence of free iodine, and contained much hydriodic acid; when dissolved in water, the addition of a little ammonia caused a deposit of crystals after some time. I made a determination of the iodine as precipitated from the aqueous solution of this substance by nitrate of silver, to obtain an idea how far it corresponded with the compound above described; it gave 46·97 per cent. of iodine in this way, approximating roughly to an hydriodate of comenamic ether, which requires 42·77. Of course, this experiment and the conclusion I draw, are only given as a rough confirmation of the existence of compounds, corresponding to the hydrochlorate.

Comenamic Ether (*Comenamethane*).—The pure ether is readily obtained from the hydrochlorate, or the residue left at 212°, as before described, by addition of oxide of silver, or, as I prefer, of ammonia, to its hot aqueous solution. The alkali is added in such quantity that the reaction remains feebly acid, and then the ether is at once precipitated in the form of colourless prismatic needles. After a little washing with cold water, one resolution in the same menstruum, boiling, is sufficient to render them perfectly pure. Their analysis was as follows:

5·012 grains, dried at 212°, gave
9·625 ... carbonic acid, and
2·377 ... water.

	Experiment.	Calculation.		
Carbon,	52·37	52·45	C_{16}	96
Hydrogen,	5·26	4·91	H_9	9
Nitrogen,	...	7·64	N	14
Oxygen,	...	35·00	O_8	64
	100·00	100·00		183

These results correspond in the most complete manner with the formula,

$$C_4 H_5 O, C_{12} H_4 NO_7,$$

which represents the ether of comenamic acid, a substance analogous to the ether of oxamic acid, the oxamethane of Dumas. On the same principle of nomenclature, the new compound may be called comenamethane; in the crystallized state, it has two equivalents of water, which it readily loses in the water-bath:

$$\begin{cases} 19{\cdot}780 \text{ grains, air-dry, lost at } 212^{\circ}, \\ \;\;1{\cdot}790 \quad \cdots \quad \text{water,} \end{cases}$$

giving a percentage of 9·04; and 8·95 is the number required by a loss of two atoms of water from the formula;

$$C_4\,H_5\,O,\,C_{12}\,H_4\,NO_7 + 2\text{ aq.}$$

Comenamethane is perfectly neutral to test-paper. It dissolves completely, but by no means easily, in boiling water and is deposited on cooling in groups of colourless prismatic crystals; it is very sparingly soluble in water at the ordinary temperature. Rectified spirit also takes it up in the heat to some extent, but absolute alcohol dissolves it sparingly in its hydrated state, and scarcely at all when dried. All the mineral acids dissolve it at once with extreme ease, nitric acid converting it after some time into acid oxalate of ammonia, which crystallizes out in beautiful rhombs. It fuses at a temperature above 400° Fahr. into a yellowish liquid, which concretes to a crystalline mass, or sometimes remains a pellucid solid when cold. It is unaltered by ammonia in the cold, and undergoes no change if it be heated, deprived of its own water of crystallization, with a solution of the dry gas in absolute alcohol, in a sealed tube, for four hours in the water-bath. I hoped, in this manner, to obtain a neutral amide, but found that the ether is unchanged under these circumstances; if water be present, the reaction results in the production of comenamate of ammonia, a substance easily identified by its reactions.

It may be remarked as a characteristic of comenic acid, that all attempts have failed to produce with it neutral salts of the fixed alkalies or ammonia, and also that no neutral ether and corresponding amide can be formed, as is the case, on the contrary, with many bibasic acids. At least, I have resorted to the most feasible methods I could devise for bringing about these results in the present instance without success.

The preceding investigation was pursued in the laboratory of Professor Anderson of Glasgow, and I cannot refrain from expressing my grateful sense of the assistance and animation I have experienced from his valuable advice, and the interest he has always taken in the progress of my researches.

In conclusion, I append a tabular statement of the substances whose composition is substantiated in this paper.

Meconamidic acid, dried at 212°, .	$6\ HO,\ C_{84}\ H_{24}\ N_7\ O_{63} + 9\ HO$
Meconamidate of ammonia, white salt, dried at 212°, .	$6\ NH_4O,\ C_{84}\ H_{24}\ N_7\ O_{63}$
Meconamidate of ammonia, yellow salt, dried *in vacuo*,	$6\ NH_4O,\ C_{84}\ H_{24}\ N_7\ O_{63} + 3NH_3 + 6\ HO$
Biamidomeconic acid, dried at 212°, .	$HO,\ C_{14}\ H_5\ N\ O$
Biamidomeconic acid, air-dry, . .	$HO,\ C_{14}\ H_5\ N_2\ O_9 + aq.$
Comenovinic acid, dried at 212°, . .	$HO\ C_4\ H_5\ O,\ C_{12}\ H_2\ O_8$
Comenamethane, dried at 212°, . .	$C_4\ H_5\ O,\ C_{14}\ H_4\ NO$
Comenamethane, crystallized, . .	$C_4\ H_5\ O,\ C_{14}\ H_4\ NO_7 + 2\ HO$
Hydrochlorate of Comenamethane, dried *in vacuo*, .	$C_4\ H_5\ O,\ C_{14}\ H_4\ NO_7,\ 2\ HO + HCl.$

A Draft Arrangement of the Genus Thamnophilus, Vieillot.

By Philip Lutley Sclater, M.A., F.Z.S.

The bush-shrikes of South America, forming the genus *Thamnophilus* of Vieillot, have been much neglected by modern ornithologists, and are at present in a sad state of confusion. Mr George Gray, in his Genera of Birds, gives a list of more than fifty species of the genus, while the Prince Charles Bonaparte in his Conspectus, under the three heads *Cymbilaimus*, *Thamnophilus*, and *Dysithamnus* (which together correspond to Mr Gray's *Thamnophilus*), reduces the number to sixteen. Excluding the four or five *Dysithamni*, which I think are fairly entitled to generic separation, I am acquainted with about thirty-six distinct members of the genus *Thamnophilus*. Three others, which I have not yet seen, raise the total number included in the following list to thirty-nine. These I believe to be all truly existing species; others which have been placed by different authors in the genus, but which I do not believe to be valid species, are the following:

1. *T. palliatus*, Lesson. Rev. Z., 1839, p. 104; Gray's Gen., i. p. 298, sp. 25. "Ater; pallio, pteromatibus, duabus lineis super alas niveis; caudâ rotundâ."—Brazil. Perhaps *Pyriglena domicella* (Licht.)
2. *T. cristatellus*, Vieill. Nouv. Dict., xxxv. p. 201; Enc., p. 750; Gray's Gen., sp. 36.
3. *T. rubicus*, Vieill. N. D., iii. 316; Enc., p. 747; Gray's Gen., sp. 40.
4. *T. guttatus*, Vieill. N. D., iii. 315; Enc., p. 746; Gray's Gen., sp. 42.
5. *T. longicaudatus*, Vieill. N. D., iii. 315; Enc., p. 746; Gray's Gen., sp. 44.
6. *T. viridis*, Vieill. N. D., iii. 318; Enc., p. 749, sp. 26; Gray's Gen., sp. 45.
7. *T. viridis*, Vieill. Enc., p. 750, sp. 33; Gray, sp. 46.
8. *T. virescens*, Vieill. N. D., iii. p. 319; Enc., p. 749; Gray, sp. 37.
9. *T. cyanocephalus*, Vieill. N. D., iii. 318, ex "*Batara obscuro y negro*," Azara, No. 217; Enc., p. 748; Gray, sp. 39.

10. *T. chloropterus*, Vieill. N. D., iii. 310; Enc., p. 742; Gray, sp. 43.

I do not think it is worth while to reprint Vieillot's characters for these nine species, his descriptions are generally so very inaccurate. The first six of them are said to be in the Paris Museum. Perhaps Dr Pucheran could succeed in recognising these as he has already so many other lost types of Vieillot and Lesson.

11. *Lanius ruber*, Gm., i. 308. *Thamnophilus ruber*, Gray's Gen., sp. 47, "body bright red!" a *Pyranga??*

12. *Lanius varius*, Gm., i. 307. *Thamnophilus varius*, Vieill. N. D., iii. p. 318; Gray's Gen., sp. 48.

13. *Lanius niger*, Gm., i. p. 301. *Thamnophilus niger*, Gray's Gen., sp. 50, is a *Tityra*, as observed by Mr Gray in his Appendix. I consider it the same as *Tityra leuconota*, Gray's Gen., pl. 63. *Pachyramphus nigrescens*, Cab. Orn. Notiz. Wiegm. Archiv., 1847, p. 241, and *Pachyrhynchus aterrimus*, Lafr. R. Z., 1846, p. 320.

14. *Lanius durantius*, Lath. Ind. Orn. i. 79. Tham. sp. 49 Gray's Gen.,—is, I have little doubt, *Lanio atricapillus*, (Gm.)

Many of the ant-thrushes have been likewise called *Thamnophili* by various authors. It is indeed difficult to see how these birds can be placed in two different families, and I agree with the late Mr Strickland,* that "the genus *Thamnophilus* cannot possibly be separated from the American ant-thrushes in any natural arrangement." M. d'Orbigny, who has had the advantage of observing these birds in their native wilds, is entirely of the same opinion, and was, I believe, the first to propose this union. He gives, in his Voyage dans l'Amerique Meridionale, Oiseaux, p. 465, a very interesting account of the general habits of the genus *Thamnophilus*. "The bush-shrikes," says this author, "are in America the representatives of our shrikes, with this important difference in their habits, that instead of being seen always *on* the bushes they keep *within* them, and rarely come to the outside. They are bush-birds *par excellence*, living in all places where dense thickets present themselves, whether that be in the neighbour-

---

* Ann. Nat. Hist., 1844, p. 415.

hood of dwelling-houses, or in deserted fields in the middle of the forests, or more often in the small woods somewhat elevate and full of thorns, called "*Chaparrales*" by the Spaniards, which are characteristic of certain parts of South America. They generally go alone or in pairs, and are quite familiar, approaching inhabited places, keeping in perpetual movement among the lower branches of the bushes, and traversing them in every direction in search of insects, larvæ, and ants, rarely descending to the ground, and then indeed only to seize their prey, to devour which they return to the lower branches of the trees. They appeared to us to be sedentary in the countries where they live, but to be always passing from one place to another. What traveller in the midst of the savage wilds, so common in America, has not, particularly in the spring-time, listened with wonder to the noisy cries of the bush-shrikes, to the sonorous strains which the males pour forth, especially in the pairing-time? Their whole body trembles with joy—their crest is raised, they open their wings, and shew every symptom of pleasure, while the female hastens to answer to their transports, though in less energetic strains. These conversations often strike the ear, but in vain one seeks what produces them, the birds being almost always hidden in thickets so dense that even the sun's rays hardly penetrate them. It is there, also, that they build their nests, some feet above the level of the earth, formed outside of sticks, and sometimes lined with horsehair within. Their eggs have much resemblance to those of our shrikes, that is to say, they are whitish, spotted with violet-red."

The geographical range of the genus *Thamnophilus* is somewhat confined, one species only, as far as I am aware, having passed the isthmus of Panama, and M. d'Orbigny says he never saw them farther south than 32° south latitude, nor on the western side of the Andes. Dr Tschudi also observes that they are not found in Transandean Peru, but in Ecuador they certainly appear on both sides of the great range, there being several specimens of two species (which I have lately described as new) in the British Museum, from the shores of the Gulf of Guayaquil. M. d'Orbingy also states that their vertical range is confined to 6000 feet above the sea-level, and we accordingly find the species most abundant near the coasts of Brazil and

Guiana, and in the valley of the Amazons, where the elevations are not great.

There is in my opinion no sufficient difference in the structure of the 39 birds described in the present paper, to necessitate their separation into smaller groups, and I have therefore merely placed the subgeneric names that have been proposed at the head of the several sections, and retained *Thamnophilus* as a generic name throughout.

The measurements of each species are given in inches and decimal parts. I have been particular in obtaining as many accurate localities as possible for each species, an important point this, which has been too much neglected in ornithology, and have always added the authorities for the localities.

DIV. A. BATARA, Lesson. *Maximi: caudâ longâ: rostro fortiore.*

1. THAMNOPHILUS CINEREUS, Vieill.

Thamnophilus cinereus, Vieill. Nouv. Dict. d'H.N. xxxv. p. 200. (♂) 1819. *Th. rufus*, Vieill. ib. (♀). *Th. undulatus*, Gray's Gen., i. p. 297; Bp. Consp., p. 197. *Th. vigorsi*, Such. Zool. Journ., i. p. 557, pl. 7 (♂), 8 (♀), 1825. *Th. gigas*, Sw. Class. Birds, ii. p. 220. *Th. procerus*, Licht.

Lanius undulatus, Mikan. Del. Fl. et Faun. Bras., pl. 2. 1820. *L. procerus*, Licht. in Mus. Berol.

Vanga striata, Q. and G. Voy. de l'Uran. Ois., i. p. 98. pl. 18 (♂), 19 (♀). 1824.

Batara striata, Less. Tr. d'Orn. p. 347.

♂ cinereus; pileo cristato nigro; dorso, alis caudâque nigris, albo trans-fasciatis.

♀ pileo antice castaneo; fasciis ferrugineis neque albis; subtus albo-cinerea, ventre brunnescente.

Long. tota 14·0, alæ 5·0, caudæ 7·0.

Hab. South-east Brazil, San Joan del Rey, and S. Paolo (Mus. Berol.); Minas Geraes (Such.); Rio Grande do Sul (Plant.)

I regret we have no information of the habits of this species, the finest and largest of the whole group. It appears to have been one of the many novelties discovered in Brazil by Delalande, and to have received its first published name from his specimens in the Paris Museum. The measurements given by Vieillot are not quite correct, but there seems no doubt that this was the bird intended by his description of *Thamnophilus cinereus*.

There is a difference of authorities as to the respective colouring of the sexes of this bird. I have taken the slate-coloured one as the male, and the brown as the female, which, judging by analogy, must be correct, though the contrary is not unfrequently stated to be the case.

2. THAMNOPHILUS SEVERUS, Licht.

Thamnophilus lineatus, Vieill. Nouv. Dict. d'H. N., iii. p. 316 (♀?). *Th. severus*, Gray's Gen., i. p. 297. *Th. niger*, Such. Zool. Journ., i. p. 589 (♂). 1825; Jard. and Selby, Ill. Orn. pl. 21. *Th. Swainsoni*, Such. Zool. Journ., p. 556, pl. suppl. 5 (♀). *Th. othello*, Less. Cent. Zool., p. 65, pl. 19.

Lanius severus, Licht. Verz. d. Doubl. p. 45. 1823.

Batara othello, Less. Tr. d'Orn. p. 347.

♂ niger unicolor, cristatus.

♀ pileo castaneo; corpore nigro et ferrugineo confertim trans-fasciato; caudâ nigrâ, obsoletè trans-fasciatâ.

Long. tota 8·5, alæ 3·5, caudæ 4·5.

Hab. South-east Brazil, San Paolo, (Licht.); Minas Geraes (Such.)

3. THAMNOPHILUS LEACHI, Such.

Thamnophilus Leachi, Such. Zool. Journ., i. p. 588 (♂); Jard. and Selby, Ill. Orn., pl. 41; Gray's Gen., i. p. 298; Bp. Consp., p. 198. *Th. ruficeps*, Such. Zool. Journ., i. p. 589 (♀). *Th. variolosus*, Licht. in Mus. Berol.

♂ ater; suprà albo ocellata; ventris pennis albido strictè marginatis; caudâ nigrâ.

♀ niger; ferrugineo ocellata; pileo ferrugineo striato.

Long. tota 10·5, alæ 3·5, caudæ 5·0.

Hab. South-east Brazil, Minas Geraes (Such.); Rio Grande do Sul (Plant.); Monte Video (Mus. Berol.)

4. THAMNOPHILUS MELEAGER, Licht.

Lanius meleager, Licht. Verz. d. Doubl., p. 46. 1823.
Thamnophilus guttatus, Spix, ii. p. 25, pl. 35, fig. 1 (♀). 1824; Max. Beit. et Nat., iii. p. 1019. *Th. maculatus*, Such. Zool. Journ., i. p. 557. pl. suppl. 6. 1825. *Th. meleagris*, Gray's Gen., i. p. 297.

♂ niger, guttis magnis albis aspersus; alis caudâque albo trans-fasciatis; subtus albus; pectoris lateribus nigris albo guttatis.

♀ guttis et fasciis fulvidis abdomine pallidè ochraceo.

Long. tota 9·0, alæ 3·5, caudæ 4·0.

Hab. South-east Brazil, Espirito Santo, Bahia and Minas Geraes (Max.); S. Paolo (Licht.)

This beautiful species may be distinguished at once by the fine tear-like spots on the black upper plumage, which extends round to the sides of the breast. Prince Maximilian says it lives singly, or in pairs and families, and is a lazy, quiet bird. He observed it always in the great woods. Its food consists of insects.

DIV. B. CYMBILAIMUS, G. R. Gray: *rostro latiore: mandibulâ inferiore turgidâ.*

5. THAMNOPHILUS LINEATUS, Leach.

Lanius lineatus, Leach, Zool. Misc., pl. 6.
Thamnophilus lineatus, Gray's Gen., i. p. 298.
Cymbilaimus lineatus, Gray's List of G. 1842, p. 49; Bp. Consp., p. 197.

♂ suprà niger, angustè albo trans-fasciatus; pileo nigro; subtùs albo-cinereus regularitèr nigro trans-fasciatus.

♀ pileo rufo, fasciis corporis superi fulvidis; infrà fulvescens fasciis minùs distinctis nigris.

Long. tota 6·5, alæ 3·0, caudæ 3·0.

Hab. Cayenne, Ecuador, prov. Quixos (Gould).

This is by no means an uncommon bird in collections from Cayenne. There were also several examples in Mr Gould's collection from Quixos, of which I gave a list in the Proceedings of the Zoological Society for 1854 (May 9th), so it would seem to have a considerable range.

6. THAMNOPHILUS LUNULATUS, Less.

Lanius lunulatus, Cuv., in Mus. Paris.—Less. Tr. d'Orn., p. 375, pl. 45, fig. 2.

♂ cristatus; suprà rufus; subtùs albo nigroque trans-fasciatus; caudâ nigrâ, albescente trans-fasciatâ.

Long. tota 8·5, alæ 3·7, caudæ 3·0.

Hab. Cayenne (Poiteau in Mus. Paris).

Of this bird I have as yet seen but one sex, which is, I rather expect, the male, and of this only the two examples in the Paris and British Museums. In the formation of the bill it approaches the preceding species, and would perhaps form a second *Cymbilaimus* for those who separate that as a genus from *Thamnophilus*.

As no accurate description of this bird has been published, I add my notes of the British Museum specimen. Crest, nape, whole back and wings, bright rufous; lores, sides of the neck, and underparts densely lineated transversely with black and white, tail dull black, outer feathers barred with whitish.

DIV. C.—TARABA, Lesson: *medii; rostro modico: caudâ breviore.*

7. THAMNOPHILUS MAJOR, Vieill.

Batara el major, Azara Apunt., No. 218, undè.

Thamnophilus major, Vieill. Nouv. Dict., iii. 313; Enc. Meth., p. 744; d'Orb. Voy., p. 166; Schomb. Reise., iii. p. 607; Bp. Consp., p. 198; *Th. stagurus*, Max. Beit., iii. 990; Gray's Gen., p. 297; *Th. albiventer*, Spix, ii. p. 23, pl. 32 (♂ and ♀); *Th. bicolor*, Sw. Zool. Journ., ii. 86 (♂); Orn. Dr., pl. 60; Gray's Gen., i. p. 297 (♀); *Th. cinnamomeus*, Sw. Zool. Journ., ii. p. 87 (♀); Gray's Gen., p. 297; *Th. magnus*, Wied. Less. Tr. d'Orn., p. 375.

Lanius stagurus, Licht. Verz. d. Doubl., p. 46.

♂ Supra niger; subtùs albus; remigibus rectricibusque; nigris albo trans-guttatis.

♀ Suprà cinnamomea; subtùs alba; tectricibus alaribus apice cinerascentibus.

Long. tota 7, alæ 3·7, caudæ 3·0.

Hab. Trinidad, Guiana (Schomb.); Brazil, Para (Wallace); Pernambuco (Spix); Bahia (Licht.); Rio Belmonte (Max.); Bolivia, Yungas Cochabamba, Santa Cruz de la Sierra, and Chiquitos (d'Orb.); Paraguay (Az.); Argentine Rep., Santa Fe, and Corrientes (d'Orb.)

8. THAMNOPHILUS MELANURUS, Gould.

Thamnophilus major, Tsch. Av. Consp. in Weigm. Archiv., 1844, p. 277, et F. P., p. 170.

♂ suprà niger subtùs albus; tectricibus alaribus apice albis; caudâ nigrâ.

♀ suprà castaneo-cinnamomea; loris et regione auriculari nigricantibus; subtùs alba.

Long. tota 8·5, alæ 3·7, caudæ 3·3.

Hab. New Granada, Bogota; East Peru, river Ucayali (Gould).

Mr Gould's collection contains examples of this bird, which differs from the preceding, in the want of the white bars on the rectrices. Bogota skins and the Ucayali specimens agree in this respect, though Tschudi describes his Peruvian "*major*" as having some white spots upon the outer barb of the external tail-feather.

9. THAMNOPHILUS TRANSANDEANUS, Sclater.

Thamnophilus transandeanus, Sclater, Pr. Z. S., 1855 (Jan. 23).

♂ suprà niger; subtùs albus; tectricibus alarum superioribus et caudæ inferioribus nigris albo terminatis; caudâ nigrâ, rectricibus duabus utrinque extimis maculâ parvâ terminali albâ.

Long. tota 8·1, alæ 3·7, caudæ 3·2.

Hab. Guayaquil in Ecuador (Barclay).

A specimen of this apparently distinct species in the British Museum was brought by Mr Barclay from Guayaquil, where it is found in the thickets. It resembles *T. major* in general appearance, but has the under tail-coverts black, with the ends terminated with white, and wants the medial spots on the rectrices, the two outer of which only have white tips.

10. THAMNOPHILUS LUCTUOSUS, Licht.

Lanius luctuosus, Licht. Verz. d. Doubl., p. 47.
Thamnophilus luctuosus, Tsch. F. P., p. 172; Gray's Gen., i. p. 297; Bp. Consp., 298 (excl. synon.) *Th. melas*, Cuv., in Mus. Paris.

♂ niger cristatus, tectricibus alarum minoribus, superioribus, inferioribus et caudâ apice albis.

♀ aut juv. cinerascentior et cristâ rubrâ.

Long. tota 6·7, alæ 3·2, caudæ 2·5.

Hab. Para (Licht.); Eastern wood region of Peru, between 12°. and 14°. S. lat. (Tsch.)

I have seen specimens of this bird in the Berlin, British and Paris museums. It has been confounded by some authors with *T. severus*, from which it is, however, quite distinct.

11. THAMNOPHILUS CORVINUS, Gould.

♂ ater; axillis summis niveis; rostro producto valido.

Long. tota 7·5, alæ 3·5, caudæ 2·7.

Hab. Eastern Peru, Riv. Ucayali (Gould.)

The only example I have seen of this bird is in Mr Gould's collection from the Ucayali. It was obtained in June 1852, and is marked "male, irides brown." Lafresnaye's *Th. immaculatus* agrees with the present species in general colouring, but is of much slighter form, and has a weaker bill.

12. THAMNOPHILUS FULIGINOSUS, Gould.

Thamnophilus fuliginosus, Gould, Pr. Z. S., 1837, p. 80; Gray's Gen., i. p. 298.

♂ cinereus; gutture et capite cristato nigris; caudâ obsoletè trans-faciatâ; rostro nigro, valido, adunco.

Long. 7·5, alæ 3·5, caudæ 3·0.

♀ summo capite, dorso alisque castaneo-fuscis; loris lineâ super oculos, plumis auricularibus, colli lateribus, gutture, corpore subtùs et caudâ intensè cineraceo-cœruleis; plumis singulis lineis cinerascenti-albis fasciatis; pogoniis internis rectricum albis lineis fasciatis; rostro pedibusque nigro-brunneis.

Hab. British Guiana.

I have seen but one specimen of this bird, a male, in the Derby Museum at Liverpool. I have therefore taken Mr Gould's characters for the female.

13. THAMNOPHILUS HYPERYTHRUS, Gould.

♀ T. suprà schistaceus; alis caudâque nigris, tectricibus alaribus albo guttatis; subtùs rubro-ferrugineus; rostro nigro.

Long. tota 7·0, alæ 3·2, caudæ 2·3.

Hab. Chamicurros, on the Peruvian Amazon. Mr Gould's bird is marked "female, irides brown." It is the only individual I have seen of the sort. Chamicurros I take to be the place marked Camucheros in the Society's Atlas, on the right bank of the Amazon above Tabatinga.

DIV. D. THAMNOPHILUS, Vieill.: *minores: rostro modico.* SUB.-DIV. A. *Radiati.*

14. THAMNOPHILUS DOLIATUS, Linn.

Lanius doliatus, Linn. S. N., i. 136. *L. rubiginosus*, Lath. Ind. Orn., Suppl. p. 18 (♀). *Piegrieche rayee de Cayenne*, Buff. Pl. Enl. 297. *Le Rousset*, Le Vail. Ois. d'Afric., ii. pl. 77, fig. 2, undè. *Thamnophilus doliatus*, Gray's Gen., i. p. 297 (partim); Schomb. Guian. iii. p. 687.

♂ niger, albo trans-fasciatus; vertice cristato nigro, medio albo; rectricibuso mnibuset in pogonio externo albo maculatis.

Long. tota 6·0, alæ 2·9, caudæ 2·5.

♀ suprà rubiginosa, pileo castaneo; subtùs valdè delutior, cinnamomea; striis quibusdam in lateribus capitis et gutture nigris.

Hab. British Guiana (Schomb.); Cayenne (Buff.); Trinidad (S.), Nicaragua and Guatemala (Bp.)

Specimens of this bird from Cayenne, Guiana, and Trinidad agree well, and must be taken for the true "*doliatus*" of Linnæus. Nor can I discover any great points of difference between these and the examples I have seen from Central America, and must therefore conclude that the range of this species extends beyond the Isthmus of Panama, though several corresponding forms represent it in the intervening countries, where this bird does not appear to exist. Schomburgk says it is one of the commonest birds of the coast of British Guiana. Its favourite resort is the thick *Avicennia* bush and damp underwood. It is an active bird, always in motion, and slips quickly through the thick bush. The male and female are always found in company. When excited they raise their crests. In the interior they are much scarcer.

15. THAMNOPHILUS ALBICANS, Lafr.

Thamnophilus albicans, Lafr. Rev. Z. 1844, p. 82; Gray's Gen., i. p. 297.

"Affinis staturâ et picturâ *Thamnophilo doliato,* a quo differt præcipuè cristâ longiore et intensè nigrâ, non basi albidâ ut in *doliato;* gastro albicante; gutture quibusdam striis minutis, pectore maculis triangularibus, ventre vittis transversis parcis et distantibus notato; abdomine medio albo. Long. 17 cent."*

I cannot quite make out this species. The describer says nothing of the coloration of the tail, whether identical with that of *Th. doliatus* or not.

16. THAMNOPHILUS CAPISTRATUS, Lesson.

Thamnophilus radiatus, Spix, Av. Bras., ii. p. 24, pl. 35, fig. 2 (♂), 38, fig. 1 (♀), nec. (Vieill.) *Th. doliatus,* Max. Beit. ,iii. 995, nec. (Linn.); ♂ Gray's Gen., i. p. 297 (partim); *Th. capistratus,* Less. Rev. Z., 1840, p. 226; Gray's Gen., i. p. 298.

♂ niger, albo trans-fasciatus; fasciis corporis inferi minùs constipatis; ventre medio albo; rectricibus lateralibus nigris, maculis solum in pogonio exteriore albis; rectricibus duabus mediis in pogonio utroque albo maculatis.

♀ capite, dorso, alis caudâque ferrugineis; subtùs pallidè flavido-rufescens; pectore obscuriùs nigro transvittato; ventre crissoque albidis. (Pr. Max.)

Hab. South-east Brazil, Minas Geraes (Max.)

This bird is certainly distinct from the *doliatus* of Cayenne, and I think also from the true *radiatus* of Vieillot, ex Azara.

It is hardly likely to be the same as any of the New Granadian species described by M. de Lafresnaye, the localities being so distant.

I have not yet seen the female, which, to judge by Prince Maximilian of Neuwied's description and Spix's figure, must be quite different from the female of *doliatus*. The male bird may be at once distinguished from the latter species by the

---

* Lafresnaye, *l. c.*

black cap, and the want of white spots on the inner barbs of the rectrices, except in the middle pair. I confess M. Lesson's description of his *T. capistratus* is somewhat too brief to enable one to assert, without fear of contradiction, that he intended this species and no other; but it is accurate enough as far as it goes, and I think it better, therefore, to use his name than to coin a new one.

17. THAMNOPHILUS RADIATUS, Vieill.

Batara listado, Azara, No. 212. v. i., p. 196, undè.
Thamnophilus radiatus, Vieill. Nouv. Dict., iii. 315. *Th. doliatus*, d'Orb. Voy., p. 168; Gray's Gen., i. p. 297, (partim); Hartlaub. Ind. Az., p. 14.

♂ pileo cristato nigro; suprâ niger albo trans-fasciatus; infrà albus fasciis angustis magís distantibus, in ventre ferè evanescentibus, nigris; gutture et crisso irregulariter albo punctatis; rectricibus omnibus et in utroque pogonio albo maculatis. Long. tota 6·3, alæ 2·9, caudæ 2·6.

♀ suprà ferruginea, pileo intensiore; infrà pallidè ochracea, gutture et ventre medio albis: lateribus capitis et nuchâ nigro densè striatis.

Hab. Paraguay (Azara), Bolivia, Yungas, Santa Cruz de la Sierra, Chĭquitōs and Moxos (d'Orb.)

The preceding characters are taken from a pair of birds in my collection, received from Bolivia. In comparing them with the true "*doliatus*," we find the following differences: *Above*, the crest is black, and wants the medial white vertical band of the "*doliatus*," and the hinder part of the neck is rather more mixed with white. *Below*, the plumage is much whiter, the sides of head are striated with black, and there are black points on the throat; the black bands on the breast are much narrower and wider apart, and grow obsolete on the belly, the middle of which is almost white. The white spots on each web of the tail-feathers are situated as in *doliatus*, but are broader and squarer in form. In the female, the plumage above agrees with *doliatus* ♀; below there are no striæ on the throat, but this and the middle of the belly are white; the breast and sides being pale creamy buff.

18. THAMNOPHILUS BREVIROSTRIS, Lafr.

Thamnophilus brevirostris, Lafr. Rev. Z., 1844, p. 82; Gray's Gen., i. p. 298.

"Nigro et latè cristatus; cristâ a basi totâ nigrâ; suprà et subtùs nigro alboque striatus, sed striis albis dorsalibus ferè squamæformibus, undulatis, striisque nigris pectoralibus distantibus; abdomine medio albo; caudâ punctis minutis striatâ. Long. tota 16½ cent. Hab. in Novâ Grenadâ, Bogota."

I can only give the Baron de Lafresnaye's description of this species, which I have not yet seen. He adds, that it is closely allied to *Th. albicans* and *multistriatus*, as well as *doliatus*, but is distinguished by its beak being shorter and more elevated at the base, by its dorsal bands being more undulated, and its tail being nearly black, traversed only by lines of very small white points.

19. THAMNOPHILUS TENUEPUNCTATUS, Lafr.

Thamnophilus tenuepunctatus, Lafr. Rev. et Mag. de Zool. 1853, p. 339.

"T. cristatus, cristâ nigrâ; suprà totus ater maculis minimis vel potiùs punctis albis quasi aspersus; remigibus atris, vexillo interno tantùm maculis triangularibus latioribus albis marginato; rectricibus totis nigris acutissimé limbo externo albo punctatis; subtùs totus nigro alboque fasciatus, colli antici pectorisque fasciis nigris paulò latioribus quasi squamæformibus; rostro nigro, tomiis apiceque albescentibus. Long. tota 14 cent. Habitat Anolaima in Novâ Grenadâ." (Lafr.)

20. THAMNOPHILUS MULTISTRIATUS, Lafr.

Thamnophilus multistriatus, Lafr. Rev. Z. 1844, p. 82; Gray's Gen., i. p. 298.

♂ suprà niger, confertìm albo trans-fasciatus, subtùs albo nigroque alternè vittatus, gutture magis striato.

♀ suprà rufo-castanea; subtùs et in colli lateribus albo nigroque crebro trans-fasciata; caudâ rufâ. Long. tota 4·8, alæ 2·8, caudæ 2·5. Hab. Santa Fe di Bogota. (Lafr.)

My characters for this species are taken from specimens which agree in all essential points with M. de Lafresnaye's descriptions. The male has no crest, and the head and whole upper surface are regularly barred across with black and white. The female, or the bird I take to be such on M. de Lafresnaye's authority, resembles the female of *Thamnophilus palliatus*, but has the bill rather smaller, and the plumage beneath much more white.

21. THAMNOPHILUS PALLIATUS, Licht.

Lanius palliatus, Licht. Verz. d. Doubl., p. 46, 1823; *L. vestitus*, Cuv. in Mus. Paris.

Thamnophilus lineatus, Spix. A. Bras., ii. p. 42, pl. 33, fig. 1 (♂), 2 (♀), 1825; Tsch. F. P. p. 171. *Th. fasciatus*, Sw. Zool. Journ., ii. 88, 1825; Gray's Gen., i. p. 297. *Th. badius*, Sw. Orn. Draw., pl. 65, (♂) 61, (♀). *Th. palliatus*, Max. Beit., iii. 1010; d'Orb. Voy., p. 174; Gray's Gen., i. p. 297; Bp. Consp., p. 197.

♂ suprà castaneus; pileo nigro; subtùs niger, albo crebrò trans-fasciatus.

♀ pileo castaneo.

Hab. South East Brazil, Bahia, (Licht.); Eastern wood region of Peru, (Tsch.); Bolivia, Guarayos and Chiquitos, (d'Orb.)

Prince Maximilian of Neuwied gives an interesting account of this bird in his *Beitrage*. He says it has a very peculiar voice, beginning high and descending through the octave in quickly succeeding tones.

22. THAMNOPHILUS TORQUATUS, Swains.

Batara acanelado, Azara. No. 215, undè.

Thamnophilus ruficapillus, Vieill. Nouv. Dict., iii. p. 318 (♀)? *Th. torquatus*, Sw. Zool. Journ., ii. p. 89, 1826; Gray's Gen., i. p. 298.

T. scalaris, Licht. in Mus. Berol., undè. *Th. scalaris*, Max. Beit., iii. 999, 1831. *Th. atropileus*, Lafr. and d'Orb. Syn. Av. in Mag. de Zool., 1837, p. 117; d'Orb. Voy., p. 173; Gray's Gen., i. p. 298. *Th. pectoralis*, Sw. Am. in Men., p. 283; Gray's Gen., i. p. 298.

♂ cinereus; pileo nigro; alis rufis; subtùs albidus; pectore nigro trans-fasciato; caudâ albo nigroque trans-fasciata.

♀ pileo rufo; alis fusco-rufo limbatis; subtus mari similis; rectricibus fuscis albo notatis.

Long. tota 5·5, alæ 2·4, caudæ 2·2.

Hab. Brazil, Bahia (Sw.); Bolivia, Chiquitos (d'Orb.)

Collections from Bahia not unfrequently contain examples of this species; which, though so well marked, has been furnished with four or five different names by modern ornithologists.

SUBDIV. B. CRISTATI.

23. THAMNOPHILUS ATRICAPILLUS, Vieill.

Piegrieche hupée de Canada, Buff. Pl. Enl. 479, fig. 2, undè.

Lanius canadensis, Lin. S. N., i. 134 (♀) certè. *L. atricapillus*, Gm. S. N., i. 303.

Le Fourmillier huppè, Buff. H. N., iv. p. 476, undè. *Turdus cirrhatus*, Gm. S. N., i. p. 826. *L. pileatus*, Lath. Ind. Orn., i. p. 76.

Tyrannus atricapillus, Vieill. Ois. de l'Am. Sept., pl. 48, p. 78 (♂), et. *Tyr. canadensis*, ib. p. 79, pl. 49 (♀).

Thamnophilus cristatus, Max. Beit., iii. p. 1002. *Th. cirrhatus*, Schomb. Reise., iii. p. 687.

♂ cinereus; dorso medio rufescenti-brunneo; capite cristato, toto cum gutture et pectore antico nigris; alis caudâque nigris albo limbatis. Long. tota 6·5, alæ 2·9, caudæ 2·5.

♀ cristâ rufâ; subtùs ochraceo-alba; gutture nigro striato; ventre medio albo.

Hab. Trinidad (Sc.); British Guiana (Schomb.); Cayenne (Sc.); South East Brazil, Bahia (Max.)

The female of this well-known bush-shrike is certainly the *Lanius canadensis* of Linne, a name which cannot be adopted on account of the error in locality. Whether Gmelin's synonyms really refer to this species is a more doubtful matter; Mr G. R. Gray applies one of them, which is used by Cabanis as a name for this bird, to a species of *Formicarius;* and I have therefore thought it better to employ Vieillot's (perhaps Gmelin's ?) " *atricapillus*" as the first-given unobjectionable name for this bird. It appears to range along the eastern shores of South America, from Trinidad to South Brazil. The next following species probably takes it place in the interior of the continent on the upper branches of the Amazon, while the *Thamnophilus albinuchalis* represents it on the opposite side of the Andes.

24. THAMNOPHILUS LEUCHAUCHEN, Sclater.

Thamnophilus leuchauchen, Sclater, Pr. Zool. Soc. 1855, Jan. 23.

♂ pileo cristato cum lateribus capitis et gutture antico ad medium pectus nigris; nuchâ cervice laterali et corpore subtùs albis; dorso murino-brunneo; alis caudâque nigris albo limbatis; rectrice unâ utrinque extimâ in pogonio externo medio et omnibus apice albo maculatis; rostro et pedibus nigris. Long. tota 6·4, alæ 2·8, caudæ 2·5.

♀ cristâ ferrugineâ; subtùs ochracea, gutture nigro striato, lateribus capitis et nuchâ ochraceis albo mixtis.

Hab. Eastern Peru, Camuchurros (Gould.)

My specimens of this *Thamnophilus* were purchased of Parzadaki of Paris, and are marked "Rio Nigro." Mr Gould's collection contains a female example from Camuchurros. It may be distinguished from the preceding species by the slightly inferior size, and weaker bill, by the bright white sides of the neck and under-parts, which are ash-coloured in the *Th. atricapillus*, the more chestnut-coloured tinge of the brown back, and the termination of the black below upon the breast instead of reaching down to the middle of the belly.

25. THAMNOPHILUS ALBINUCHALIS, Sclater.

Thamnophilbus albinuchalis, Sclater, Pr. Z. S., 1855, Jan. 23.

♂ suprà murino-brunneus; nuchâ albâ; dorso medio albo mixto; capite summo cristato nigro; alis fuscis, tectricibus albo limbatis; caudâ nigrâ, rectricum omnium apicibus et unæ utrinque extimæ margine externo albis; subtùs albus; gutture et pectore antico nigris; capitis lateribus albo mixtis. Long. tota 6·5, alæ 3·2, caudæ 2·5.

♀ suprà brunnescentior capite et caudâ totâ rufo-ferrugineis; nuchâ et corpore infrà ochraceis.

Hab. Guayaquil (Capt. Kellett in Mus. Brit.); island of Puna (Barclay in Mus. Brit.).

The British Museum contains the only examples I have seen of this *Thamnophilus*, which seems to take the place of the preceding species on the shores of the Pacific. It may be distinguished from both of them by its broad white nape, and the mixture of white feathers in the interscapularies.

26. THAMNOPHILUS MELANONOTUS, Sclater.

Thamnophilus melanonotus, Sclater, Proc. Zool. Soc., 1855, Jan. 23.

♂ Niger; interscapularibus albo mixtis; dorso postico cinereo; abdomine cinerascenti albo; alis nigris albo marginatis; caudâ nigrâ, rectricibus omnibus apice et extimâ utrinque laterali etiam pogonio externo medio albo-maculatis; rostro et pedibus nigris. Long. tota 6·5, alæ 3·0, caudæ 2·5.

Hab. Santa Martha, on the north coast of New Grenada (Verreaux).

A single specimen of this species in my collection was sent by the MM. Verreaux's collector from Santa Martha. It is closely allied to the three preceding, but may be at once distinguished by its black back.

27. THAMNOPHILUS ASPERSIVENTER, Lafr. et d'Orb.

Thamnophilus aspersiventer, Lafr. et d'Orb. Syn. Av. in Mag. de Zool., 1837, p. 10; d'Orb. Voy., p. 171, pl. 4. fig. 1 (♂), fig. 2 (♀), (err. sub nom. *Th. schistacei*); Lafr. Rev. Zool. 1844, p. 83; Gray's Gen., i. p. 298.

♂ niger; dorso cinerascente; ventre toto cinereo nigroque asperso; alis caudâque et dorso medio albo notatis.

♀ abdomine et caudæ tectricibus inferioribus rufis.

Long. tota 6·5, alæ 3·0.

Hab. in Boliviâ, Yungas, Sicasica, et Ayupaya (d'Orb.); Novâ Grenadâ (Lafr.)

SUBDIV. NÆVI.

28. THAMNOPHILUS NÆVIUS, Gm.

Spotted Shrike, Lath. Syn., i. pt. 1, p. 190, undè.
Lanius nævius, Gm. S. N., i. p. 308; Leach, Zool. Misc., t. 17.
L. punctatus, Shaw, G. Z., viii. pt. 2, p. 327.
Le Tachet Le Vail, Ois. d'Afr., ii. pl. 77, fig. 1, undè.
Thamnophilus nævius, Sw. Orn. Dr., pl. 59; Schomb. Reise., iii. p. 687. *Th. albonotatus*, Spix. Av. Bras., ii. p. 27, pl. 37,

fig. 2 (♂). 38, fig. 2 (♀). *Th. cærulescens*, Lafr. R. Z., 1853, p. 338.

♂ cinereus; pileo summo et dorso medio nigris, hoc albo mixto; alis nigris albo limbatis; caudâ nigrâ, rectrice unâ utrinque extimâ maculâ pogonii exterioris mediali et omnibus maculâ apicali albis.

♀ pallidè viridescenti-rufa, subtùs valdè dilutior, ventre albicantiore; pileo ferrugineo; remigibus nigricantibus externè brunneo limbatis; rectricibus brunneis; his et alarum tectricibus et secondariis, sicut in mari, albo notatis.

Long. tota 5·5, alæ 2·7, caudæ 2·1.

Hab. Cayenne (Sc.), British Guiana (Schomb.); North Brazil, Para (M. B.); Bogota (Sc.)?

The *Lanius nævius* of Gmelin is founded upon Latham's "*Spotted Shrike*." The describer says of this,—"The tail is black, all the feathers tipped with white, and on each of the outer feathers is a spot of white on the outer web about the middle of each feather." These characters and the habitat clearly indicate the present bird, in contradistinction to the South-east Brazilian *T. ambiguus*, which the Baron de Lafresnaye, in a recent article in the Revue et Magazin de Zoologie has considered as the true "*nævius*." His discovery of the distinctness of that bird from the present is by no means novel, the same having been clearly set forth in the Zoological Journal for 1827 by Mr Swainson, and he has, besides, assigned names to the two species that cannot be retained, the present bird not being, as I believe, the *cærulescens* of Vieillot, and the *T. ambiguus* not identical, as I have before observed, with the true *nævius* of Gmelin.

Besides my Cayenne examples, I have seen many North Brazilian specimens which I refer to this species. They differ, however, from the Cayenne birds, as well as from one another, in the amount of white edgings on the secondaries, and spots on the upper tail-coverts; as also in the belly being darker cinereous, and in some (which I consider younger birds) obsoletely barred across, but agree always in the markings of the tail.

Nor do I venture at present to separate the Bogota variety as a distinct species, though, in the specimens I have seen from that locality, the bill is stronger, the black head extends farther down the nape, and the under plumage of a much darker tinge.

29. THAMNOPHILUS CÆRULESCENS, Vieill.

Batara negro y aplomado, Azar. No. 213; ii. p. 199, undè.
Bat. pardo dorado, Azar. ii. p. 202, No. 214 (♂), undè.
Thamnophilus cœrulescens, Vieill. Nouv. Dict. iii. 311 (♂). *Th. auratus*, Vieill. l. c. p. 312 (♀). *Th. nœvius*, Gray's Gen., i. p. 297 (pars.), d'Orb. Voy., p. 170?

Hab. Paraguay (Azara); Bolivia, Chiquitos (d'Orb.)

The account given by Azara of this bird seems to agree best with the true "*nœvius*." As, however, we have here several closely allied species, to all of which a loose description is equally applicable, I am unwilling at present to attach this to any of them, and propose to leave it by itself until the examination of specimens from Paraguay and Bolivia shall afford the means of clearing up the doubt.

30. THAMNOPHILUS VENTRALIS, Sclater.

T. cinereus; fronte, pileo, nuchâ et dorso medio nigris, hujus pennis internè niveis; alis nigro-brunneis, primariis strictè albo limbatis; tectricibus alaribus nigris albo terminatis; rectricibus nigris, duabus mediis exceptis, albo terminatis; unæ utrinque extimæ pogonii externi dimidio apicali albâ, maculâ ovali subapicali nigrâ; subtùs albo-cinereus, ventre medio crissoque albis lateribus subcinerascentioribus; mandibulâ superiore pedibusque nigris, inferiore plumbescente.

Long. tota 6·2, alæ 2·8, caudæ 2·6.

Hab. South Brazil.

The greater amount of black upon the head, and whiteness of the middle of the belly and crissum, as also the want of white edgings to the secondaries, distinguish the bird above described, of which I possess one specimen, from *Th. nœvius* and *ambiguus;* but the chief peculiarity which I rely upon for its being undoubtedly separable from those birds consists in the colouring of the outer pair of tail-feathers. The white spot on the outer web of these, instead of being confined to a small, nearly square space, as in the two other species, here reaches down to where the black terminates on the inner barb, leaving only a small, oval, black spot between it and the broad white termination of the feather.

31. THAMNOPHILUS PILEATUS, Swainson.

Thamnophilus pileatus, Sw. Zool. Journ., ii. p. 91.

"T. suprà cinereus, infrà pallidior, europygio pectorisque lateribus fulvis; vertice nigrâ; remigum fuscarum margine testaceo; rectricum acutarum apicibus lineâque marginali albis.

Long. tota 6·0, alæ 2·7, caudæ 2·5." (Swains.)

Hab. Brazil, Catinga woods of Bahia.

Mr Swainson compares this species with *T. ambiguus*, from which it seems to differ in the markings of the tail-feathers. As to these being *pointed* at the extremities, to which fact Mr Swainson appears to attribute much importance, I do not think that can always be relied on as a valid distinctive character. I have as yet seen no bird I could recognize as this species.

32. THAMNOPHILUS AMBIGUUS, Swains.

Thamnophilus nævius, Vieill. N. D., iii. 316; et Enc. Meth. p. 747; Lafr. Rev. et Mag. de Zool., 1853, p. 338. *Th. ambiguus*, Sw. Zool. Journ., ii. p. 91; Gray's Gen., i. p. 298. *Th. nigricans*, Max. Beit., iii. 1006; Gray's Gen., i. p. 218. *Th. ferrugineus*, Sw. Zool. Journ., ii. p. 91 (♀)? Gray's Gen., i. p. 298.

♂ cinereus; subtus albescentior; pileo dorsoque medio nigris, hujus pennis internè albis; tectricibus alarum caudæque superioribus et rectricibus nigris albo terminatis, his omnibus prætereâ in utroque pogonio medialiter albo notatis; primariis angustè, secondariis latiùs extùs albo limbatis.

♀ virescenti-cinerea, subtùs pallide fulva; pileo rufo; tectricibus alaribus et secondariis nigris, horum margine externâ, illarum apice albis; primariorum marginibus et rectricibus brunneis, his albo terminatis.

Long. tota 5·7, alæ 2·8, caudæ 2·3.

Hab. South-east Brazil (Max.); Minas Geraes (Such.)

Vieillot's *T. nævius* appears to be intended for this species, though he professes to copy his characters from Latham. Mr Swainson, however, has clearly put forward the distinctions between the true *nævius* and the present bird; and the Prince Maximilian of Neuwied has described it with his usual accuracy under the title of *nigricans*, and gives a lively account of its habits. He says it is one of the commonest of the whole family in Brazil.

33. THAMNOPHILUS NIGROCINEREUS, Sclater.

Thamnophilus nigrocinereus, Sclater, Proc. Zool. Soc., 1855, Jan. 23.

♂ cinereus, capite toto, cum dorso summo et gutture nigris; interscapularibus basi albis; alis caudâque nigricantibus, albo limbatis; rectrice utrinque extimâ mediâ albo notatâ rostro et pedibus nigris.

Long. tota 5·75, alæ 3·8, caudæ 2·4.

♀ rufo-brunnea; gulâ et ventre medio albescentioribus; alarum tectricibus secondariis et caudâ sicut in mari albo notatis.

Hab. North Brazil, Para (Mus. Brit.)

A pair of birds of this species in the British Museum were received from Para, and I have a male in my own collection which I believe to be from the same locality. It differs from the *Th. nævius* and its near affinities in the much larger size, stouter bill, and black throat. The quills are brownish-black, narrowly margined exteriorly with white. The upper and under tail-coverts are partly tipped with white.

34. THAMNOPHILUS MACULATUS, Lafr. et d'Orb.

Thamnophilus maculatus, Lafr. et d'Orb. Syn. Mag. de Zool., 1837, p. 11; d'Orb. Voy., p. 172; Lafr. Rev. et Mag. de Zool. 1853, p. 339.

♂ suprà griseo-ardesiacus; pileo summo nigro; interscapuliis albo mixtis; tectricibus alaribus et caudæ rectricibus nigris albo terminatis; harum unâ utrinque extimâ etiam pogonio externo medio albo notatâ; capitis lateribus, gutture et pectore pallidè griseo-cærulescentibus; ventre crissoque rufescentibus.

♀ suprà grisescenti-olivacea; pileo summo et uropygio rufescentibus; subtùs magis rufescens; alis caudâque nigricanti-brunneis rufescente limbatis.

Long. tota 6·0, alæ 2·8, caudæ 2·5.

Hab. Corrientes, in the Argentine Rep. (d'Orb.)

I have one specimen, which I believe to be an immature male of this species, in my own collection, and have seen others. It may be distinguished, as M. d'Orbigny observes, from *Thamnophilus nævius*, which it resembles in the markings of the tail feathers, by the want of white edgings to the secondaries, and its rufous belly. In his characters for this species in the Rev. et Mag. de Zool. for 1853, p. 339, the Baron de Lafresnaye omits all notice of this last very distinctive character; indeed he says "*colore subtùs intensiùs* ARDESIACO;" and I cannot help thinking, therefore, that he was referring to some other bird, possibly to the true "*cærulescens*" of Vieillot.

35. THAMNOPHILUS RUFICOLLIS, Spix.

Thamnophilus ruficollis, Spix. Av. Bras., ii. p. 27, pl. 37, fig. 1; Schomb. Reise., iii. p. 687.

Mediocris; fuliginoso-cinereus; corpore subtùs, capite colloque rufis; tectricibus alarum caudæque apice albo-marginatis (Spix).

Hab. Brazil (Spix), British Guiana, lower bush of the coast woods (Schomb.)

The only bird I have seen likely to belong to this species, described by Spix and recognized by Schomburgk, is one in Mr Gould's collection from Chamicurros, which may be characterized as follows:—

Virescenti-cinereus; capite toto cum corpore subtùs rufis, ventre dilutiore; alis nigris, tectricibus omnibus et secondariis albo latè limbatis; primariis externo margine brunneis; alis subtùs ochraceis; caudâ nigrâ, rectricibus omnibus apice et unâ utrinque extimâ pogonio externo medio albo notatis; interscapuliis quibusdam albo mixtis.

Long. tota 5·7, alæ 2·7, caudæ 2·3.

36. THAMNOPHILUS MACULIPENNIS, Sclater.

Thamnophilus stellaris, Spix, Av. Bras., ii. p. 27, pl. xxxvi. fig. 2?; Sclater, Pr. Z. S., 1854, P. (May 9, certè); *Th. maculipennis*, Sclater, M.S.

♂ plumbeus subtùs clarior; pileo dorsoque medio nigris;

interscapuliis subtùs niveis; tectricibus alarum apice albo guttulatis; caudâ brevi; rostro plumbeo.

Long. tota 5·5, alæ 3·0, caudæ 1·8.

♀ subrufescenti-grisea; alis rufis; fronte capitis lateribus et corpore subtùs pallidè rufescenti-brunneis; gutture clariore; lateribus griseo mixtis.

Hab. Quixos in Cisandean Ecuador and Chamicurros, on the Peruvian Amazon (Gould).

I have seen several examples of this bird, of both sexes, from the upper Peruvian Amazon and adjoining countries. In the Paris Museum are specimens collected in those parts by Messrs Castelnau and Deville in 1847 (No. 1121, Voy. 863). I have usually taken it to be the *Thamnophilus stellaris* of Spix, and used that appellation for it in my list of the birds in Mr Gould's Rio Napo collection. Other authors, however, have united Spix's name to the bird called *Myiothera plumbea* by Prince Maximilian of Neuwied, which belongs to the genus *Dysithamnus*. Spix's somewhat loose description and imperfect figure are nearly as applicable to one bird as the other. His locality, *Para*, does not suit the present species. I have thought it better, therefore, to avoid confusion by giving this bird a new name. An examination of Spix's type specimen in the Munich Museum, if still existing, will decide whether I am right in doing so or not.

SUBDIV. D. OBSCURI.

37. THAMNOPHILUS CÆSIUS, Sclater.

Lanius cæsius, Cuv., in Mus. Paris. *Thamnophilus cæsius*, Sclater, Pr. Z. S. 1855, (Jan. 23).

♂ nigro-plumbeus; pileo cristato gulâque nigris; tectricibus alarum angustè albo limbatis; caudâ nigricante unicolore; rostro pedibusque nigris.

♀ grisescenti-brunnea, cristâ nigricante; capitis lateribus, tectricum alarum marginibus et corpore subtùs rufis; rostro nigro, mandibulâ inferiore basi et pedibus pallidis.

Long. tota 5·5, alæ 3·25, caudæ 2·25.

Hab. British Guiana.

Two specimens of this bird in my possession were selected from a large collection of birds from British Guiana which contained many similar. The examples in the Paris Museum, the only other place where I have met with this species, are marked *Lanius cæsius*, Cuv., which is, I believe, merely a MS. name.

38. THAMNOPHILUS SCHISTACEUS, d'Orb.

Thamnophilus fuliginosus, Lafr. et d'Orb., Syn. Av. in Mag. de Zool., 1837, p. 10; d'Orb. Voy., pl. 5, fig. 1. *Th. schistaceus*, d'Orb. Voy., p. 170.

♂ "totus schistaceus, obscurus; subtùs pallidior; rectricibus lateralibus albescente limbatis; rostro pedibusque cæruleis."

"Long. 6 poll" (Lafr.)

Hab. Bolivia, Cochabamba (d'Orb.); New Grenada (Lafr.)

One specimen of a bird which I refer to this species is in the British Museum.

39. THAMNOPHILUS IMMACULATUS, Lafr.

Thamnophilus immaculatus, Lafr. Rev. Z. 1845, p. 340. *Th. campterii*, Gray's Gen., iii.; App. p. 14.

♂ ater; summi pennis quibusdam niveis.

Long. tota 6·5, alæ 3·3, caudæ 3·0.

♀ brunneo-cinnamomea; fronte loris gutture genis caudâque totâ nigro ardesiacis; rostro pedibusque nigris mandibulâ albicante (Lafr.)

I have a male specimen of this bird sent to me by the MM. Verreaux. M. de Lafresnaye does not mention the white feathers at the upper end of the carpal joint; but the wings in Bogota skins are so squeezed up into the body that this slight white patch is very likely to escape notice, and I have little doubt that the birds are identical.

On the Production of Boracic Acid and Ammonia by Volcanic Action. By ROBERT WARINGTON, F.C.S.

The simultaneous occurrence of boracic acid and ammonia in the neighbourhood of volcanoes has been frequently observed, and its cause has given rise to a good deal of speculation, although no very definite conclusions have as yet been arrived at. Some information and specimens I have received from a friend who visited the Island of Vulcano, which is situated about 12 miles north of Sicily, have enabled me to make a few experiments, which, though not so complete as I could have wished, appear to throw some light upon this point. My friend supplies the following information:—"The height of the volcanic mountain is estimated at about 2000 feet, and its crater is about 700 feet deep. The area at the bottom, which may be about 10 acres in extent, is covered with small, loose pieces of limestone, just as though it had been macadamized, and the ground is so hot as rapidly to destroy the leather of the shoes. On thrusting a thermometer between the stones, it indicated, at different points, temperatures varying from 250° to 500° Fahr. On looking over this area from the top of the crater, one side of it appeared as if covered over with beautifully-white drifted snow. On reaching the spot, however, this white appearance was found to be caused by a deposit of finely-crystallized boracic acid. On removing this incrustation, which formed a layer of about an inch in thickness, and digging with a pick-axe, there spumed up a mass of red-hot fused lava, similar in appearance to the slag of a glass-house; this consists of fused saline matters in cohesion with volcanic debris. In other parts of the crater there are holes like foxes' holes, from which blue jets of volcanic flame are issuing continually, and a deposition of sulphur occurs all around.

"The boracic acid rises in vapour, and condenses on the surface of the ground at the bottom of the crater like a light drifted snow; and when gathered up, the surface becomes covered again with sublimed acid in two or three days. To ascertain this point more decidedly, some hogshead casks, having their heads removed, were filled with broom-plants and twigs, and were placed over parts of the area from which the boracic acid had been carefully cleared away. In a few days the acid had been vaporized into them, and had deposited in crystals like hoar-frost all over the twigs. On digging down for about eight inches, wherever this boracic acid occurs on the surface, a red-hot mass of sal-ammoniac is always found; sulphur comes up also with these.

" This volcano is said to realize to the proprietors about £1000 per annnm. The products are sulphur, from fusing the stone; sal-ammoniac, from the lixiviation of the scoria or lava; and boracic acid, large quantities of which are reported to be obtained annually from this source. The sides of the volcano are of sulphur-stone, and brimstone is dug up all around for miles. The mountains produce also alum, which exists in the schistose rocks; and there are likewise large beds of lignite; but nowhere do we find sal-ammoniac or boracic acid, either at Vulcano or in Tuscany, separate from one another. Had they done so, we should certainly have found traces of it somewhere, but, so far as I know, this has never been observed; and it is certain that, at Vulcano, whenever the acid lying on the surface is removed, the melted matter underneath is found to contain salts of ammonia. It follows, therefore, that they must both be produced from one and the same stratum, in which they occur in some form of combination, from which they are separated by heat. In what substance can they exist together?"

These observations of my friend were accompanied by specimens of the sublimate scraped from the surface of the crater, and of sal-ammoniac, which have enabled me to do something towards the solution of the question with which he terminates his letter. The ammoniacal salt was not a portion of the fused mass mentioned above, but had been obtained by its lixiviation and subsequent crystallization. I did not, therefore, attempt to make any experiments with it. The boracic acid, however, was in the state in which it was found, and had the form of white glistening scales of a nacreous lustre, tinged in parts with traces of adherent sulphur, and possessing a greasy talcose feel. It was, in the first instance, boiled with diluted hydrochloric acid, allowed to become clear by subsidence, and the solution decanted from the undissolved portion. The latter was washed, to remove the adhering acid, and boiled with a weak solution of caustic potash, without the least trace of ammonia being liberated. The residue was collected, washed with distilled water, and dried. Some caustic potash was next fused in a tube of hard glass, and, while in this state, was found to yield no evidence of ammoniacal gas. A fragment of the dried, white, insoluble residue was then dropped into the potash, and the fusion repeated. Strong evidence of the formation and liberation of ammonia was at once indicated. It was obvious, from this experiment, that the ammonia could not have been really formed in this substance, but must have been produced by some decomposition effected by the potash. These phenomena at once recalled to my mind the interesting compound of boron and nitrogen, discovered in the year 1842, by Mr Balmain, who applied to it the name of Ethogen, and which has since been examined by Professor Wöhler. This compound is produced by heating borax and ferrocyanide of potassium, in their anhydrous states, to a full red-heat in a covered crucible. The white, infusible, porous mass, which results from this action is washed with a large quantity of boiling water, acidulated with hydrochloric acid.

The nitride of boron so obtained is insoluble in water and acids, even when concentrated, but when fused with caustic potash, ammonia is copiously evolved, and if heated in a current of steam to a moderate red heat, it is entirely converted into boracic acid and ammonia. These characters correspond with those of the white compound I have examined, as far as the evolution of ammonia is concerned, but owing to the small quantity at my disposal, I was unable to determine the presence of boracic acid, or rather of boron, except by its peculiar phosphorescence before the blowpipe flame. The existence of this compound in active volcanoes would also explain, in a satisfactory manner, the simultaneous presence of boracic acid and ammonia. I am in hopes of obtaining some of the fused mass which lies below the surface of the crater, and should I do so, I may be able to establish some additional facts, which may form the subject of a future communication.

On the Principal Depressions on the Surface of the Globe.

By Dr George Buist, Bombay.*

In the following admirable digest, extracted from the Bombay Times (Sept. 28th 1854), it is apparently merely for the sake of simplicity, and for a convenient starting point, that the author supposes that " the earth assumed its present character and conformation," either by having " risen directly," or " through a long series of elevations," so that the streams that drain its surface, and the waters in its inland hollows, are the result of one or of a series of actions of upheaval, accompanied, however, by " stupendous disturbances and frightful distortions amongst the rocky beds;" which " must have occurred at the time of their elevation," these being followed by " change and commotion" on a minor scale, examples of which occasionally occur, even down to the present day.

Since the revival of the doctrines of Hutton, geologists have been gradually abandoning the idea of vast disturbances and changes, caused by the exercise of forces more sudden and stupendous than those of which we have experience; and it is held by many, that the surface configuration of existing continents is the result of the complicated action of numerous gradual upheavals and depressions, and long-continued marine and atmospheric denudations; during which, through the various epochs of geological time, the same mountain chains were formed by repeated disturbances, strong, though slow in their operation. Hence, some of them in their earlier stages, formed the nuclei of existing continents, while other ancient ranges and tracts of land of continental extent, now form at least part of the bed of the ocean. The existing drainage of the world is therefore not simply the result of recent great changes of the outlines of the terrestrial surface; but the origin of many of our systems of drainage, and perhaps even in some cases of individual rivers, must be sought for in disturbances connected with geological epochs, often far removed. The same is true in a minor degree of areas of depression.

---

* Read to the Bombay Geographical Society, Sept. 14, 1854.

When the crust of the whole earth, or any portion thereof, first assumed its present character and conformation, it must necessarily have been devoid of rivers until a sufficiency of rain fell to moisten its surface, fill up its hollows, and occasion an overflow; the surplus water passing off in the form of rivulets, brooks, streams, or rivers, to the nearest lower level, and so downward till they found their way to the sea. If we assume the dry land all at one time to have been submerged, and all to have risen directly, either at once or through a long succession of elevations, to its present level, such of the spaces as were depressed below the surrounding country at the time of their emergence, and that so continued, would of course be filled with salt water; and would probably thus remain, either until evaporation converted it into a mass of solid salt, or until, washed down to the sea by the rains, its place came to be occupied by pure water. In many places, as will presently be seen, fragments of the primeval ocean remain in the bosoms of our continents in nearly the condition in which they originally appeared. Though the most stupendous disturbances and frightful distortion amongst the rocky beds must have occurred at the time of their elevation, there can be no doubt that change and commotion continued long after this, and that ridges, hills, and mountains rose, chasms were split open, and valleys sunk everywhere in multitudes throughout the whole lapse of intervening time; examples of such things occasionally occurring in volcanic countries down to our own day.

Just 280 years before Christ, the great fresh-water lake of Oitr in Japan was formed in one night by a prodigious sinking of the ground, at the same time that one of the highest and most active volcanoes in the island rose into existence. The volcanic peak of Jurullo, on the table-land of Mexico, 70 miles from the Pacific, rose on the night of the 29th September 1759, 1683 feet above the plain, and is the highest of six mountains that have been thrown up on the table-land since the middle of last century. In July 1757 a volcanic island arose off Pondicherry, near Madras, and, after remaining for several days above the water, throwing out smoke and flame, disappeared. About the same time Chedooba, and the islands along the shores of Arracan, were suddenly raised about ten feet, having twice before, at intervals, as is supposed, of half a century, sustained similar upheavals. In 1762, during a violent earthquake, a mountain sank and disappeared near Chittagong, in the upper part of the Bay of Bengal; another descended till the summit alone remained visible, while 60 square miles of sea shore were permanently submerged. In 1831 a volcano called Graham's Island rose on the coast of Sicily to the height of 800 feet, and, after continuing in active conflagration for three months, sank down and vanished beneath the waters;* and in June 1819, the Runn of Cutch, in our own neighbourhood, sank down, and became a salt-water marsh—a vast mound, called the Ulla Bund, rising in its neighbourhood, and cutting off from the sea one of the mouths of the Indus. The island of Bombay and plains of the Deccan must at one time have been on the same level with each other.

So soon as rain began to fall, all the hollows would be filled up, and transformed into lakes, either with rivers running into them, or out of them, or both. Our great river systems now first make their appearance, and connect in long reaches of nearly stagnant water the original hollows, now transformed into lakes united together by rapids and cataracts. In process of time the more shallow and inconsiderable of these pools would become filled up with mud or gravel, assisted by the hitches and upheavals to which the crust of the earth from the first seems to have been periodi-

---

* This volcanic cone was formed principally of ashes and scoriæ. There is no proof that it sunk; but when the further supply of material ceased, the loose matter was quickly washed away by the waves.—(EDIT. *Phil. Jour.*)

cally subjected, forming our haughs, carses, and holms; the only depressions remaining permanently as lakes being those near the sources of rivers, where the feeders that supplied them, being inconsiderable in size, brought comparatively little solid matter along with them, rendering the process of filling up infinitely slow. All our lakes, however, are in process of gradual obliteration, more solid matter being carried into them than finds its way out; and all that is required is a sufficient lapse of time to accomplish their extinction, when those at the sources of our streams will undergo the transformation into plains and levels which their predecessors along their tracks have already undergone. The depth of many of our lakes is very great indeed, the bottom of their basins being often very far below the level of the sea; so that, were their supplies of water diminished, or the evaporation from their surfaces increased, we should have examples presented us, wherever this prevailed, parallel to that with the lakes of Asphaltites, Assal, Tiberias, the Caspian Sea, and many others, of a pool of entirely salt water at the bottom of a hollow lower than the level of the sea; and to this class of hollows only do we give the name of depressions.

The bottom of Loch Ness, and of some of the other lakes along the line of the Caledonian Canal, are not only below the level of the surface of the German Ocean, but beneath that of its bed anywhere in the line of their axis across to the shores of Norway.

Were the Straits of Babelmandel closed, the Red Sea would be all but dried up in a moderate lapse of years, presenting us with a huge chasm, in some places half a mile in depth, with a long, narrow bitter lake, margined with rock-salt at the bottom.

The following are some of the dimensions of the most notable of our lakes:—

Names.	Area.	Elevation of surface.	Depth.	Bottom below Sea.
	Sq. Miles.	Feet.	Feet.	Feet.
Geneva	240	1,230	1,012	...
Superior	32,000	672	932	300
Ontario	...	279	547	268
Titicaca	2,225	12,846	720	...
Tiberias	50	*—329	165	494
Dead Sea	185	—1,312	1,300	2,612
Caspian Sea	140,000	—82	...	82

I shall turn next to the great continental river basins, or valleys of no outlet, where the rivers on all sides flow towards some central lake or lakes, and the whole of their waters are carried off by evaporation. These may be classed under two divisions—those above, and those beneath, the level of the ocean; and the first we must note of the first class are those of America—the most notable being that of the Great Salt Lake

* Mrs Somerville says, in a note on these depressions, that the level of Tiberias as given by actual measurement of Symonds is not to be relied upon, as it falls short by above 100 feet of that determined barometrically by three different observers—Berton, Russerger, and Von Wildenbruch, who give the mean at 755; the mean assigned to the Dead Sea by the traveller is 1423·5. With great deference to so distinguished an authority as Mrs Somerville, I should certainly prefer the most ordinary levelling over so moderate a distance to the best barometric measurements where there could be no good barometer of reference to fall back upon. The hour of the day might make all this difference—the barometer read at 10 or 4, without a corresponding reading at the same level at exactly the same hour, would give an error of 100 feet.

of the Rocky Mountains, which, as will by-and-by be seen, in many points closely resembles the Dead Sea. The Great Salt Lake, until then chiefly familiar to us by name from the Mormon settlement on its borders, was first explored by the American Government in 1847, by an expedition under Fremont, which seems to have been mainly one of general inspection. A second expedition, under Captain Stansbury, U. S. Engineers,* laid down a base of six miles near the lake, and made an elaborate and careful trigonometrical survey of the whole district. It is situated betwixt the 42d and 43d parallels,—about the 115th western meridian,—in the bosom of the Rocky Mountains, betwixt the Missouri and the Pacific. Vast inhospitable tracts of country prevail to the north and south of it; on the east, for the space of nearly 1000 miles, are the trackless and barren steppes of the Rocky Mountains—a similar extent of salt desert bordering it to the west. The place where the Mormons have taken up their abode is one of the most isolated and extraordinary the world contains, remarkable for its beauty and fertility on the very borders of the most unspeakable desolation. The Valley of the Salt Lake is about 4000 feet above the level of the sea, and is about 500 miles either way in extent. This space, which is enclosed by a circle of rugged precipices and majestic mountains, consists of great stretches of salt desert, perfectly smooth and level, bearing all the marks of marine origin. Some of these are from 60 to 70 miles across; and they are separated from each other by precipitous rocky eminences of great elevation. On the slopes which bound the plain are a series of thirteen distinct terraces or beaches, the highest of them being about 200 feet above the valley, and to all appearance the margins of a former sea which had subsided by intervals, and left behind it the marks where it had for a time remained at rest. There are many valleys and recesses amongst the Rocky Mountains with terraced slopes similar to those just described, having all the appearance of the basins of former seas. Within the basin, but at a much higher level, besides the Great Salt Lake itself, is the fresh-water lake Utah, from which flows a stream of considerable magnitude, on which the name of the Jordan has been bestowed, and which, after passing the Mormon settlement, discharges itself into the Salt Lake. The Salt Lake itself is nearly 300 miles in circuit, including all its indentations, and is about 70 miles in length and 20 in breadth. It is studded with mountain islands, springing up abruptly from the surface of the water to altitudes of from 500 to 1000 feet, Antelope Island rising to the height of 3000 feet; eminences of similar form and size, which had been islands before the waters shrunk within their present dimensions, being scattered about over the adjoining plains. The waters of the lake contain 22 per cent. of saline matter, or about the same quantity as the Dead Sea. Of this, 20 per cent. is pure chloride of sodium or sea salt. It is said to throw down in summer muriate of soda, and in winter sulphate of soda or glauber salts—a circumstance that seems so strange that better evidence than we possess is requisite before the fact can be accepted as established. They are so acrid as to be dangerous to animal life, and even so affect and corrugate the throat when swallowed that a mouthful would be fatal. They are so heavy that the body floats on them without effort, about a sixth of its mass remaining above the surface. The lake itself is singularly shallow; its greatest depth is 33 feet, and in some places a stiff breeze blows the water alto-

---

* I have not been able to refer to the American works themselves (they are in none of our libraries), but take my information at second hand from the *Athenæum*, Oct. 1852; *Jameson's Journal*, 1852; and *Chambers's Journal*, 1853. A good outline of the Salt Lake is reserved for future works on physical geography.

gether to one side, and leaves large expanses of the bottom bare. At no distant period the lake seems to have been many times its present size, and to have covered the low lands around with its waters. It seems still diminishing in size, the balance betwixt fall and evaporation not having as yet been attained in a climate where little rain falls, and the atmosphere is intensely dry. Amidst all its stern grandeur, the scene around is one of dreary and oppressive desolation. There is no tree or plant to relieve the eye; the atmosphere feels hot and suffocating, and the sluggish waves scarcely ripple before the breeze. Along one side of the lake the surface of the earth is covered with a sheet of solid salt of the most dazzling whiteness; this is converted into a muddy marsh by every shower of rain. Various streams of fresh water flow into the lake from the neighbouring mountains—the Jordan, Bear River, and Weber, being all of considerable size; and the banks of these before they enter the salt region are covered with the richest vegetation. Hot springs and salt in masses abound in the neighbourhood of the lake. Around its margin is a band of soft, fœtid, slimy mud, consisting entirely of the larvæ of insects, or other animal matter, emitting smells the most offensive that can be imagined. All around are evidences of volcanic action, and thick cakes of mud, six or eight inches in diameter, charged with sulphur, and erupted in a semi-liquid form from small spiracles beneath, are found scattered about. In the plain, at no great distance from the lake,* is a group of volcanic cones and apertures covering several acres of ground, with steam and mud issuing from at least half a dozen chimneys. The cones are from four to six feet in elevation, terminating in a spiracle or vent, some of which are hardened, and lined with crystals of sulphur and other substances. From one of these steam and water are thrown from ten to fifteen feet into the air; they rush out with a noise resembling the escape of a steam engine; the water is hot and cold by turns, and is strongly impregnated with sal-ammoniac. Some of the cauldrons are from ten to twenty feet in diameter, filled to within three or four feet of the top with boiling mud, which occasionally runs over. Besides the numerous mud cones, there is one of lava, in the midst of a mass of volcanic rocks within the valley. It is about 50 feet in height; sheets of salt, strongly impregnated with sal-ammoniac, surrounding its base. In the mountains, not far off, are wells of petroleum and naphtha.

If I have bestowed more space on the Great Salt Lake than I ought to have done, or than time will allow to devote to other depressions of equal interest, it is because it has but lately become known to us; and I am not aware of any single paper or work in which all the information that has been collected regarding it is to be found in moderate compass. As already mentioned, the latest of our physical atlases and physical geographies fail to bring our information down to this point. I have no doubt it will be treated with his usual care and ability by my friend Mr Keith Johnston, in the new edition of his great work now preparing for the press.

There are, besides the valley of the Great Salt Lake, whose mere magnitude is the point of least interest about it, two depressions, or continental river basins of no discharge north of Mexico, on the highlands betwixt the Gulf of California and Rio del Norte; one of about 200 by 50 miles, betwixt the 29th and 33d parallels; another about four times this size, nearly under the tropics. Both contain salt lakes of some magnitude, with fresh-water streams flowing into them. Beyond this, little is known regarding them. The Rio Grande, about 300 miles in length, is the largest river in this quarter swallowed up by evaporation; and but for

---

* American Annual of Scientific Discovery for 1852; *Jameson's Journal*, No. 105, p. 180.

these continental streams the country would be doomed to a state of perpetual sterility—a few showers occurring in September being all the rain that ever falls in the neighbourhood.

In the great Andes plateau in South America, stretching from the Tropic of Cancer northwards for the space of 1200 miles, with a mean breadth of 200, is a depression with a surface area equal to about that of the Red Sea. This basin is about 12,000 feet above the level of the ocean, the principal lake being that of Titicaca, occurring at an altitude equal to that of Teneriffe. It is about 26,000 square miles in area, and 700 feet in depth. The scenery and verdure around seem in the highest degree rich and beautiful, and the climate delightful.

There are no continental river basins or valleys of any extent in any part of Europe, the rains being sufficiently abundant, and evaporation moderate enough, to enable the moisture which falls to accumulate in the valleys till it forms lakes which discharge their waters into rivers, all finding their way to the sea; and the only depressions at all resembling those under consideration, and of the same character, though of inconsiderable depth, and due, doubtless, to the same causes, are those in Holland—the Lake Harlaem and the Zuyder Zee.

We know so little of Central Africa that we are unable to speak of its characteristic features with anything like certainty. From the magnitude of some of the lakes known to exist, and the streams made mention of, compared to the scantiness of the discharge of fresh water into the sea, there is reason to believe in continental river basins great in number and vast in size. The only depressions well known to us are those of the lake Mareotis, on the Mediterranean shore, close by Alexandria, of the Bitter Lakes in the Isthmus of Suez, like Mareotis, and the Natron Lakes, all in Lower Egypt, and Lake Assal, off the shores of the Gulf of Aden, a short way into Abyssinia. The first of these depressions has probably been seen by most of those who have made the journey overland. It seems to have been formed by a sinking of the Delta up to close upon the shore, where a barrier was left; it is at its lowest some six or eight feet below the Mediterranean, and occupies an area of about 5000 square miles, being about 30 across and 150 in length. It seems to have been a fresh water marsh in Pliny's time, when the Nile was admitted to it by canal, and it was transformed into a lake. By the end of last century it had become nearly dried up, and its ancient bed, remarkable for its fertility, was irrigated by canals from the Nile. In 1801, during the siege of Alexandria, then held by the French against the English, a letter was found on the body of General Roitz, expressing alarm lest the sea should be admitted to the lake Mareotis, and the town deprived of fresh water. The hint was taken by the British General, and the barrier cut across. The vast plain was immediately submerged, the sites of 300 villages were flooded, and one of the most fertile and profitable portions of Egypt—the very garden of the Nile—reduced to sterility. For 10 or 15 miles the railway skirts or traverses the margin of the lake, so as to bring it within the view of overland passengers betwixt Europe and the East. Near the period of low Nile the waters of the lake are concentrated by evaporation up close to the point of saturation, and vast sheets of salt of dazzling whiteness, the reflection of which is seen in the sky far out at sea, spread over the shallows round its borders, to be redissolved when the waters of the Nile are admitted during the inundation. A benevolent government or enterprising people would speedily pump out the brine by steam, and restore the soil to its wonted fertility by repeated washings from the Nile. As matters at present stand it is likely to remain for ages, until the Nile silts it up to the level of the sea, a monument of the cruelties wars of aggressions inflict or compel, and of the apathy and indifference of an administration which makes no attempt to heal the wounds after they have been inflicted.

The Bitter Lakes occupy a series of hollows about 30 miles in length, 10 in breadth, and 50 feet in depth, under high water mark in the narrow neck of land intervening betwixt the Red and Mediterranean Seas. They seem at one time to have formed the upper portion of the Gulf of Suez, which was cut off from them by the rising of the desert barrier of about 13 miles, which now divides them. The water now found in them is extremely salt and bitter—the result of concentration. The isthmus, which is only 70 miles from sea to sea, seems within the last 4000 years to have been subjected to frequent elevations and depressions, the latest of which in all likelihood occurred a considerable time after the Exodus.

The Natron Lakes, in the upper part of the Delta, are also completely isolated, and occupy a depression of considerable but uncertain depth. In summer they are nearly saturated with salt, the muriate and subcarbonate of soda, or the sea-salt and soda of commerce. In winter they rise, and become freshed, from the percolation of the waters of the Nile, which appear to take about three months to force a passage through the porous soil beneath.

Before noticing Palestine, close by the locality just described, we shall close the account of the known depressions in Africa with a notice of the lake of Assal, on the Somali shore opposite Aden. The lake was, I believe, first surveyed by the party of Sir W. Harris, in 1841; it is described by him, as well as by Dr Kirk and Captain Barker, who took its level and dimensions. It is in lat. 11° 33′ 12″ N., long. 42° 30′ 6″ E. It is about 7 miles in length, 16 in circumference; and its surface is 570 feet beneath the level of the sea. No stream or rivulet enters it, or flows from it; scarcely any rain ever falls in its neighbourhood; its waters dried up and concentrated by evaporation, have nearly reached the point of saturation, and about one-third of the lake is at certain seasons covered with a sheet of solid salt. It is separated from the outer sea, of which it at one time formed a part, by a barrier of lava, cracked and rent in all directions, the whole being obviously the result of recent volcanic agency, accomplished, probably, when the vast group of cones extending from Aden 500 miles into Abyssinia, and at least 300 up the Red Sea, were in a state of conflagration. Under operations so violent and extensive as may then be supposed to have been in progress, the upheaval of a barrier a few dozens of miles across, and severation from the sea of a lake about the size of the island of Bombay, would appear a very trifling affair.

Turning from Assal I shall take up the depressions in India, few and inconsiderable as they are, before dealing with those of Western and Central Asia. The most noticeable are the Runn of Cutch, the Boke, the Null, and Lake Loonar. The remarkable thing about the first of these is that it has obviously been subjected to a variety of descents and upheavals within the human or probably historic period. Any one who reads the Periplus with care, will, I think, come to the conclusion that a vast space from the Indus eastward which is now dry land was in the time of Alexander covered by the waves. There is a Hindoo tradition that the sea in days of yore swept over the present Runn and extended for many miles beyond it, and a line of positions along the old sea margin indicate by their names the ports, custom-houses, and other chief points along the shore. A saint offended with the wickedness of the people cursed the land, and ordered the sea to retire, an event believed by Colonel Grant to have occurred in the eleventh century. The ruins of the city of Bhalibapoora near Bhownuggur are now found from 10 to 15 feet below the surface of the soil; but the houses it is clear must have been constructed on dry land, and sunk beneath the waves for at least the distance just named, when a fresh upheaval brought the whole up to its present position. The Runn of Cutch now vastly circumscribed in its area from the time of the holy man's malediction, was to a considerable extent submerged by the earthquake of the 16th of June 1819, of which sufficient mention has already been made, and now forms in part a lake, in part a salt water marsh. Considerably to the north of this in the Collectorate of Ahmedabad are two remarkable hollows some way from each other, called the Null and the Boke. They both appear the results of volcanic agency, the water they contain is salt, they receive supplies from rivulets but give off none. The only other hollow in India of any note is the basin of Lake Loonar, a depression situated among the Shiel Hills in the centre of the Deccan. It is about 500 feet below the level of the surrounding country, and seems to be the crater of an extinct volcano, lava being in abundance at no great distance. The water it contains is nearly saturated with subcarbonate of soda, the Natron of the lakes of Egypt.

We now come to the consideration of the largest and most wonderful depression in the world,—that of the north-east of Asia,—not including that of the Dead Sea, an account of which will be given last. From the borders of the Gulf of Finland and the Black Sea to those of the Yellow Sea, extending all across Central Asia, there is a space nearly 4000 miles from east to west, and at its western extremity nearly half as much from north to south, comprising in all an area of above three millions of square miles, containing lakes and rivers numberless, but which send not one drop of water to the ocean, evaporation subliming into the air all the moisture that appears on the ground. In the western portion of this the ground sinks in some places above 80 feet beneath the level of the ocean, affording a vast space of from 760,000 to 800,000 square miles in area, or larger than the Mediterranean, to all appearance the basin of an old inland sea, at no time more than slightly connected with the Northern Ocean. This depression comprehends the whole of Trans-Oxonia, including the basins near its lowest part, of the Aral and Caspian, the surface of the latter being 83 feet beneath the Mediterranean. From his observations on these points Humboldt arrives at the following wonderful but far from improbable conclusions:—

"1. That before the times which we call historic, at epochs very near in point of time to the latest revolutions on the surface of the globe, the lake Aral may have been entirely comprehended in the basin of the Caspian Sea, and that then the great depression of Asia (*the concavity of Tauran*), may have formed a vast interior sea, which may have communicated on one side with the Euxine, and on the other side, by means of cracks more or less wide, with the Icy Sea, and the lakes Telegoul, Talas, and Balkhache.

"2. That even in the historic times, we must not admit too generally that the soil has followed the successive changes which seem to be indicated by the chronological series of opinions emitted by ancient historians and geographers. These authors seldom represent the geography of their epoch:—they choose between preceding opinions, and their absolute silence respecting certain facts or natural phenomena is no argument against the xistence of these phenomena.

"3. That very probably from the time of Herodotus, as at the epoch of the Macedonian expedition, the Aral formed but a lateral appendage of the Oxus, and that it communicated with the Caspian only by the arm which the Scythian Gulf of that sea extends so far to the coast, and receives the river Oxus.

"4. That either by the simple phenomenon of the increase of growth (the preponderance of evaporation over aqueous supply,) or by plutonic crevices or elevations, the Scythian Gulf (the Karabogas) has been progressively contracted in its narrowest dimensions, and that by the retreat of the gulf the bifurcation of the Oxus has been developed—that is, has become more and more manifest. One portion of the waters of the Oxus has preserved its course towards the Caspian by a river bed which modern travellers (posterior to the middle of the 16th century) have found dried up. What was at first but an enlarged appendage of a lake, which communicated laterally with the Oxus, has become the limit of the inferior course of this river. It is thus that Nature on a great scale has repeated the phenomenon, which the hydraulic systems of the Yaryakchi exhibit to the E. and N. E. of the Aral, of the Tchoui, and Talas, terminating, after a course of 130 or 160 leagues, in the lakes of Telegoul, Kabankoulak, and Talasgol."

By far the most profound and striking, if not the most extensive depression on the surface of the globe is that of the Lake of Asphaltites or Dead Sea in Palestine. The most remarkable characteristics of this lake were well known to the ancients, and it is described by Deodorus, Pliny, Strabo, and Josephus, and though never surveyed with anything like tolerable care it has for long formed a favourite resort for travellers. Lieutenant Symonds, of the Royal Engineers, in 1843, measured its depression by actual levelling, and found the surface of its waters to be 1312 feet below those of the Mediterranean. Lieutenant Lynch, of the United States Navy, crossed and recrossed it repeatedly in 1847, taking soundings as he went. He confirms the researches of Symonds, and he speaks of having made astronomical and barometrical observations, but gives us no results; and wonderful to relate, while we organise expeditions to examine the icy seas at an expense of hundreds of thousands of pounds,—send parties into Central Africa to search for we know not what,—mount the fearful table-lands of the Andes, and survey with philosophic care the sacred lakes of the Hindoos, hid deep in the bosom of the Himalayas,—permit officers to assume all sorts of disguises, and practise every variety of questionable deception to be enabled to violate the sanctity of the great Mohammedan shrine, and to inspect that which it is deemed sacrilege for the unbeliever to behold, and is not worth describing even if it could be legitimately seen,—we are content with merely looking at a spot of earth which has more claims on our curiosity as Christians, as well as geographers and philosophers, than any point on the surface of the globe. There is not—to our shame be it spoken—up to this moment anything like a decent or even a creditable account of the physical geography of Palestine in print! and the vague and general account of it now about to be given, gleaned from all the best authors on the subject, meagre and unsatisfactory as it is, is half guess-work. This most discreditable want it was my purpose next spring to have endeavoured to some extent to have remedied, by taking the levels from Akaba down to the Dead Sea, and so up again by the Valley of the Jordan, and to the sea level, and surveying then all round by the old sea margin by a circuit of probably some 400 or 500 miles. The fulfilment of this purpose, not unlikely to be deferred for the present by another and a very different variety of geographical operations, will, I trust, be resumed should I ever be permitted to revisit my native country.

The Dead Sea is supposed at one time to have united with the eastern limb of the Red Sea, known by the name of the Gulf of Akaba. A sloping valley of unknown elevation, called the Wadi Araba, the highest part of which forming the barrier which separates the two, is somewhere betwixt 60 and 495 feet above high-water mark, and this is supposed to be within 25 or 30 miles of Akaba, the total distance betwixt the two seas being 106 miles. The fact of the Dead Sea being very much below the Mediterranean, as well as the existence of an enormous depression, enclosing and surrounding it, was known to the ancients, who conferred on the name of Hollow Syria. One of the first surmises of its enormous depth was given in 1841 by Sir David Wilkie, who made it 1200 feet by barometrical observation—probably the extent to which his barometer was cut. Two years afterwards Lieutenant Symonds made it, by levelling, 1320·2 feet, and this is now the admitted depression. Lieutenant Lynch, in 1847, fathomed water to the depth of 1300, so that the hollow is in all 2620 feet below the surface of the sea. The bottom of the sea consists of two submerged plains, one 13 feet and the other 1300 feet, at an average, below the surface. The area and upper borders of the hollow, indicated in all likelihood by an old sea margin, and to which the waters would again rise were a canal, as has been proposed, cut into it from the Mediterranean on the one side, and Red Sea on the other, are unknown to us. Along the axis of the lake and valley of the Jordan, from the water-shed in the Wadi Araba to Cesarea and Philippi, is probably 190 miles, with a bifurcation of about 210 miles to the eastward, terminating about Mount Hermon, where the streams run in opposite directions. Its greatest breadth appears to be about 30 to 45 miles, and the area of the whole depression, which is very irregular in form, perhaps somewhere about 7000 square miles. The lake itself is about 40 miles by 9, with a probable area of 185 square miles; its circuit, including all its indentations, seems about 420. The rocks around on the west side seem to be mainly of the chalk formation, mixed with old volcanic basalt, and occasionally to all appearance with recent lavas. Close by the lake, about one-third along from the northern shore, are masses of yellowish limestone, with great beds or pillars of rock salt; and the whole soil, and bottom of the lake, are covered with saline incrustations, petroleum oozing from the beach, and spreading itself in many places in films over the surface. Pieces of sulphur lie scattered around—whether the products of a volcano, or the results of the decomposition of the salt does not appear. Near the mouth of the Jordan hot springs abound. Around the northern shore, and especially manifest in the basin of the Jordan, are horizontal lines or terraces of alluvial matter on the mountains, terminating in abrupt declivities of sand, which lead again to lower terraces or beaches closely resembling those of the ocean, with here and there conical hills, with flat horizontal tops, all obviously the result of aqueous action. From these and other circumstances it is inferred that the Dead Sea was depressed to its present level, not by simple evaporation, but by the sudden sinking of its bottom sufficiently indicated by the abrupt breaks down in the bed of the Jordan. If the original theory be correct that the Dead Sea was at one time connected with the Gulf of Akaba, it is very probable that the ridge of the Wadi Araba may have risen when some of the convulsions occasioning or deepening the depression occurred, just as the Ulla Bund arose when the village of Sindree, and the portion of the Runn of Cutch around descended in June 1819. There is not the slightest reason to associate any of these convulsions, which must have been on a scale vast enough to destroy all animal life, with the destruction of the cities of Sodom and Gomorrah, and the surface of the country in the days of the patriarchs was probably not dissimilar to what it is at the present day.

Dr Graves enumerates a number of points in which the Great Salt Lake of America and the Dead Sea resemble each other. They are both situated in deep valleys, the mountains surrounding them being marked with terraces or old sea margins—proofs of a succession of sudden sinkings in the earth beneath. The shores of both abound with deposits of salt, with petroleum, and with sulphur; near both are hot springs, and other volcanic phenomena. In the valleys of both are fresh-water lakes—Tiberias in the one and Utah in the other, through which flow the rivers Jordan, in both cases losing themselves in the salt-water lakes. They closely resemble each other both in area of surface and dimensions of basin. The waters of the two are almost equally heavy, and equally salt, though they differ entirely in the nature of their saline contents, as will be seen below; and they are most unlike each other in matter of depth.

	In 1000 grains of water.		
	Dead Sea.* sp. gr. 1·22	Great Salt Lake. sp. gr. 1·17	Common Sea Water. sp. gr. 1·027
Chloride of Magnesium,	145·8	......	......
,, Calcium,	31	......	......
,, Sodium,	78	200	......
,, Potassium	6	......	25
Other Salts,	6	20	5
	266·8	220	30

It will thus be seen, that though in all likelihood the great American lake owes its saltness to the rivers washing away the salt from the rocks around, and carrying it down to be concentrated as in a great salt pan, a like explanation by no means suffices for the saltness of the Dead Sea, whose ingredients are wholly different from those composing rock or common rock or sea salt.

Depths of the Sea.—I have confined my observations to the depressions on the surface of the dry land, chiefly dealing with those which were either beneath the level of the adjoining countries, and not filled up with water, or those receding far beneath the surface of the ocean both embo-

---

* We have retained in the text the analyses given by Dr Buist, though they are very imperfect. We append more complete analyses of sea water, of the waters of the Dead Sea, and of that of the Elton Lake, described by Gustav Rose, in his "Reise nach dem Ural." This lake, and the numerous brine pools which exist in the neighbourhood of the Caspian, complete the analogy of that district, with that of the Dead Sea, and the Great Salt Lake referred to in the preceding paragraph.

	Dead Sea Water.	Common Sea Water.	Elton Lake.
Chloride of sodium,	65·77	26·72	131·24
... magnesium,	105·43	3·23	105·42
... calcium,	28·94	...	...
... potassium,	13·98	1·28	2·22
... aluminum,	0·18	...	...
Bromide of magnesium,	2·51	...	0 07
.. sodium,	...	0·51	...
Sulphate of lime,	0·88	1·62	...
... magnesia,	...	1·97	16·65
Silica,	0·03	...	...
Water,	782·27	964·67	744·40
	1000·00	1000·00	1000·00

It is worthy of observation, that the Dead Sea water and that of the Elton Lake closely resemble in composition the mother liquor which is obtained when sea water is evaporated, so as to separate the greater proportion of its common salt. That the Dead Sea is such a mother liquor, seems to be indicated by the abundance of rock-salt which is found in the neighbourhood. If, therefore, the explanation of the saltness of the Great Salt Lake given by Dr Buist is correct, the whole difference between the two is, that in the case of the Dead Sea, the concentration is further advanced.—*Edit. Phil. Journal.*

soming lakes in their depths, both receiving supplies of river water, but yielding none. The channel of the ocean in the structure and in the diversity of its surface seems in all respects closely to resemble that of the dry land, which has itself, indeed, at no distant period occupied its depths, and still bears on its surface loads of marine remains. Our lesser islands are but the summits of mountains whose bases rest on the valleys or tablelands far down in the main, presenting at times slopes as smooth and gentle, and precipices and cliffs as lofty, rugged, and abrupt as any of those made visible to the eye of man. The sounding-line discloses hills, mountains and valleys, with chasms and recesses as diversified and remarkable as any which the regions exposed to the upper air supply, covered with a dense and varied vegetation, and thickly peopled with numberless races of stirring inhabitants, to some of which in point of size the giants of the superterrene animal kingdom—the elephant, the giraffe, the rhinoceros, and the hippopotamus—are but pigmies. The mean level of the whole solid land above that of the sea is 1000 feet—that is, were our mountain masses smoothed down, and our valleys and sea margins brought up to one general table-land, its surface would be 1000 feet above that of the ocean. The mean level of Asia is 1150 feet, of that of Africa we know nothing, that of Europe 670, and that of America 930 feet, North America being 750, and South America, 1130. The mean depth of the ocean, again—that is, of its basin, were this scooped out, and smoothed in the floor till it resembled a tank or cistern, is about 22,000 feet or four miles. It has been measured to the depth of nearly seven miles, or about 36,000 feet, and it covers three-fourths of the surface of the globe. Were the solid part of the earth, therefore, to be removed, and thrown into the sea, the highest mountains would fall short by 10,000 feet of filling up its deepest recesses, and the whole mass would be submerged to the depth of a mile at least.

Vast as these inequalities are when represented in figures, the relation they bear to the diameter of the earth is insignificant. On that magnificent three-feet globe now before you, on which the hand might cover the whole space anything like tolerably known to us, the highest of the Himalayas would be represented by a grain of sand, and the enormous-looking depressions just described, by a scratch which would little more than penetrate the varnish—so very small a way beneath the surface does our knowledge extend, and our research penetrate. Yet this thin film in space furnishes the habitation of all the vegetable and animal tribes that have been formed, and the examination of the minutest portions of it taxes to the uttermost the intellect, and occupies and exhausts the energies of man.

On the Action of Gallic and Tannic Acids in Dyeing. By F. Crace Calvert, F.C.S., M.R.A. of Turin, Professor of Chemistry at the Royal Institution, Manchester.

Persoz, in his *Traité de l'impression des Tissus* (vol. i. page 262), remarks, that " It is desirable, as much for the interest of the manufacturer as for that of science, that we should know positively whether it is gallic or tannic acid which plays the most important part in dyeing with gall-nuts." This statement of Persoz, together with the knowledge of the fact that the manufacturers of extracts of colouring matters, were prevented from preparing extracts of tannin masses, by the rapid change which these extracts undergo, induced me to make the following researches, with the hope of throwing some light on the subject.

The first experiments were made with the view of ascertaining the action of gallic and tannic acids in the dyebeck. For this purpose, I dipped 100 square inches of iron-mordanted cloth into baths composed of 20 grains of these acids, and 1½ pint of water; and the dyeing was allowed to go on in the cold for 24 hours. It was found that the gallic acid rapidly dyed the iron mordant, but the colour soon disappeared, whilst with the tannic acid, although the black was slower in forming, it remained permanent. Similar trials were then repeated, but gradually raising the temperature of the bath during 1¾ hours to 180° Fahr., and then for half an hour to 212°. The general results were similar, the only difference being, that the black at first formed with the gallic acid, more rapidly and completely disappeared than in the experiments done at natural temperature.

These facts led me to believe that the gallic acid acted as a reducing agent on the hydrate of peroxide of iron fixed in the cloth as a mordant. To substantiate this view, I took a portion of the liquor from the bath in which the dyeing process had been conducted, and on examination found it to contain a large quantity of protoxide of iron in solution; whilst in the case of the tannic acid liquor, no reduction of the oxide of iron had taken place. I also added a small quantity of hypochlorite of lime to the above solution of gallate of protoxide of iron, which not only precipitated a certain quantity of the black gallate of iron, but the liquor gave a permanent black on a fresh piece of iron-mordanted calico, leaving no doubt that the hypochlorite had maintained the iron of the mordant in the state of peroxide. A very important question now presented itself, viz., will the presence of a free acid increase the reducing power of gallic acid? To determine this point, a weak solution of persulphate of iron was mixed with some gallic acid, and it was found that in proportion to the excess of acid, so did the blue precipitate first formed rapidly disappear, leaving in the glass vessel a brown-tinted liquor, containing a salt of proto and peroxide of iron. It was also ascertained that the addition of a small quantity of weak hydrochloric, sulphuric, or oxalic acids, greatly increased the reducing action. If, on the contrary, an excess of pure hydrate of peroxide of iron was added to a solution of gallic acid, even after several days, the dark-blue precipitate at first produced remained permanent, and no protoxide of iron was produced in the solution. If heat, however, was applied to the mixture, protoxide of iron might be detected in the liquor.

These facts clearly show, that gallic acid cannot be employed as a dye when used in excess, or in presence of any other acid. Whilst tannic acid placed in similar circumstances to the above described with gallic acid, does not reduce the peroxide of iron, either at natural temperatures, or under the influence of heat. The only circumstances in which the conversion of the hydrate of peroxide of iron into protoxide was remarked, were on the addition of large excesses of hydrochloric, sulphuric, or oxalic acids. I am inclined, therefore, to believe, that, under the influence of a great excess of mineral acid, the tannic acid splits up into sugar and gallic acid, and the latter substance produces the reducing effect above described.

These results seem to afford an explanation of the fact observed some years ago by M. J. Girardin of Rouen, that, to obtain good blacks, a calcareous water is advantageous, a result which is probably due to the lime of the carbonate neutralizing the gallic acid existing in the tanning matter, and so preventing it from exerting its reducing action on the iron mordant, which would interfere with the dyeing properties of its tannic acid.

I was also anxious to ascertain the difference of the action of gallic and tannic acids on alumina. I accordingly took two pieces of calico, of 100 square inches each, previously mordanted with alumina, aged and dunged, and placed them in separate baths, one of which contained 20 grains of gallic acid, and the other 20 grains of tannic acid; and during 2¼ hours gradually carried the whole to the boiling point. These pieces were then taken out, washed in distilled water, and subsequently dyed with madder. It was found that the piece which had been in the gallic acid bath was almost colourless; while that from the tannic acid had acquired a deep red tint. The same results were obtained on dyeing a piece of calico mordanted with alumina, in a bath, composed of 1½ pint water, 12 grains peachwood, and 8 grains garancine, together with 20 grains tannic, or 20 grains of gallic acid. To leave no doubt as to the true action of these acids on alumina, I introduced into two tubes pure hydrate of alumina, with a solution of each of them; and after a few days' contact, I found, on examining the supernatant fluids, that the gallic acid alone had dissolved alumina, the tannic acid not having acted at all, so that the latter may be considered, if not a neutral substance, at all events a very feeble acid.

I also attempted to obtain reds and blacks, with an extract of sumach, which had been kept some time, but failed, owing no doubt to the transformation of its tannin into gallic acid, as the results obtained were identical to those furnished by the above free acids. This rapid transformation of tannin into gallic acid in the extract of sumach, is remarkable, when it is remembered that it takes only a few weeks or months in the case of the extract, whilst it requires years when the tannin is confined in the plant. These differences are no doubt due to the presence of water, which facilitates chemical actions. This rapid deterioration of tanning matters in the form of extract is the reason why their substitution for the solid substances themselves has not been adopted by the silk dyers or tanners. I therefore deemed it advisable to make a series of experiments, in the hope of discovering a substance, which would act as an antiseptic to this peculiar fermentation, for the researches of Messrs Delaroque and Robiquet junior have clearly shown that tannin is transformed into gallic acid, and a substance resembling sugar, under the influence of a peculiar ferment called pectase.

My investigations led me to discover three substances, which possess the property of preserving from fermentation, tanning extracts having a specific gravity 1·250, and as I trust that the employment of these substances will facilitate manufactures and cheapen production, I do not hesitate to publish them. They are chloride of lime, bichloride of mercury, but especially carbolic acid, and to show the efficiency of this acid, I may add, that I have an extract of *sumach*, which was mixed, twelve months since, with a few per cent. of this acid, and which is as sound as when mixed. The first two substances answer very well, but the last has the great advantage of not interfering with the general applications of the extract of tanning matters.

The remarkable power of dissolving the hydrates of oxides of iron and aluminum possessed by gallic acid, induced me to try its action, as well as that of tannic, on metallic iron. For this purpose, 1000 grains water, 25 grains acid, and 100 grains iron-wire were introduced into tubes, so arranged as to convey the gases evolved to the pneumatic trough, care being taken to exclude all air. After a few days, it was found that several cubic inches of gas had been given off from the gallic acid tube, which, on testing, proved to be nearly pure hydrogen, whilst the liquor remained colourless, and only assumed a slight blackish-blue tint, when exposed to the air. The iron on being taken out, was carefully dried and weighed, and found to have lost 1·4 grains. Therefore, gallic acid has the property of dissolving iron. It was also observed, that in the case of the tannic acid, no gas was evolved, neither was iron dissolved; although the solution had assumed a slight purple tinge, which I attributed to some trace of oxide produced on the bright surface of the wire, during its weighing. I also tried a similar series of experiments, substituting for the 1000 grains water, 1000 grains of a solution of sugar, having a specific gravity of 1·090; and observed, that although gallic acid acted in the way above described, tannic acid on the contrary, under the influence of the sugar, attacked the iron and gave a bulky dirty purple precipitate. I regret that I had not time to examine the nature of this action, neither the peculiar compound formed by the oxidation of the gallic acid, when brought in contact with an acid persalt of iron; but if circumstances permit, I propose to return to this subject.

In conclusion, from the facts contained in this paper, there can be no doubt, that tannic acid is the constituent of tanning substances, which produces blacks with iron mordants. *Secondly*, That the reason why gallic acid produces no black dye is, that it reduces the peroxide of iron of the mordant, forming a colourless and soluble gallate of protoxide of iron. *Thirdly*, That gallic acid has the property of dissolving hydrate of alumina, and also of separating alumina mordant, from the cloth on which it is fixed. *Fourthly*, That the reason why extracts of tanning matters lose their dyeing properties is, that the tannin is transformed into gallic acid. *Fifthly*, That gallic acid possesses the property of dissolving iron, and thus lays claim to the character of a true acid, whilst tannin not having this action, appears to me to be in reality a neutral substance. *Sixthly*, That carbolic acid possesses the property of preventing the tannin-fermentation, or the conversion of tannin into gallic acid and sugar, or a similar substance, under the influence of a peculiar ferment called pectase.

On the Geologic Range of the Pterygotus problematicus.

By the Rev. W. S. Symonds, F.G.S.

" One of the strangest organisms of the formation," says Mr Hugh Miller, in " The Old Red Sandstone," " is a fossil lobster of such huge proportions that one of the average-sized lobsters, common in our markets, might stretch its entire length across the continuous tail flap in which the creature terminated."

This crustacean is the " Seraphim" of the Forfarshire quarrymen, and was for a long time supposed by Agassiz to be a " fish." Mr Hugh Miller gives an interesting account of the restoration of the "*lobster*" by the great ichthyologist himself.

Nearly allied to the Scotch fossil and the recent Limulus of the West Indies, is the Pterygotus problematicus of the Silurian rocks of England, and the object of this paper is to draw the attention of geologists to the remarkable *range* of that crustacean, and the association of a highly-organized Entomastracan with groups of fossils so widely separated as are the trilobites and mollusks of the "Caradoc conglomerate" from the ichthyolites of the "Old Red Sandstone."

In the collection of the Malvern Natural History Field Club is a portion of one of the "thoracic feet," discovered by Mr John Barrow, in the Caradoc conglomerate of Eastnor Park. This fossil is alluded to by Sir R. Murchison (Siluria, p. 237), to whom the circumstance of its detection was communicated by the late Mr Hugh E. Strickland. It is associated with Lingula crumena, Lingula attenuata, Arca Eastnori, and Pterinea orbicularis.

Another "thoracic foot" was found by the writer of this notice at the *base* of the *Upper* Ludlow shales at Gorstley Common, Newent, Gloucestershire, and was examined and named by Mr J. W. Salter. Rhynconella Wilsoni occurs in the same rock!

The fine specimen of the limbs of this Palæozoic crustacean in the cabinet of the late Mr H. E. Strickland, has been fully described (Quart. Journ. Geol. Soc., Nov. 1852, vol. viii.) by Mr J. W. Salter. The *locale* of this fossil was in close proximity to the Upper Ludlow "bone bed" of Hagley Park, near Hereford; and it was discovered associated with Avicula retroflexa, Orthis lunata, and Orbicula rugata by the late Mr Mackay Scobie.

A few weeks ago I examined a fine collection of the remains of Pterygotus in the cabinet of Mr R. Banks of Kingston, from the "tilestones" of Bradnor Hill. One of the claws of this animal is superior to the fossil of Hagley Park, while thoracic feet, spines, and the plates figured (Sil. System, Pl. IV., 4 *a*), occur in great abundance. The only fossil hitherto detected in the "tilestones," with the remains of Pterygotus, is Lingula cornea. The "Arbroath paving-stones" of the Old Red Sandstone contain numerous fossils of the same crustacean (Siluria, 247). Thus we have a range for the Pterygotus from the Caradoc conglomerate to the Devonian rock of Arbroath inclusive—a range even greater than that of the long-lived Calymene Blumenbachii.

Notice of Shoals of Dead Fish observed on the passage between Mirimachi, New Brunswick, and the port of Gloucester. Communicated by the Rev. W. S. SYMONDS. *With some Remarks by* Sir W. JARDINE, Bart.

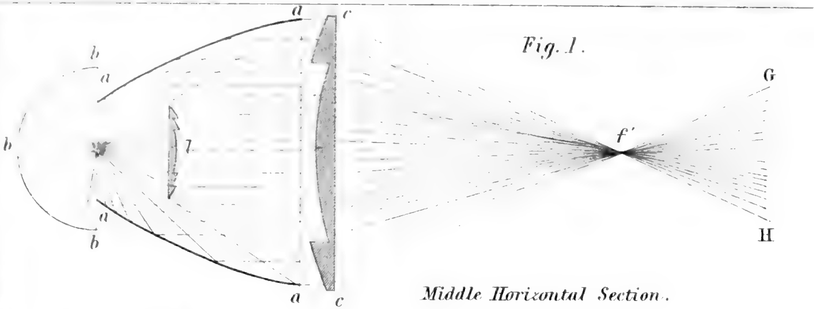
The following is an extract from a letter received from the Rev. W. S. Symonds of Pendock Rectory, Gloucestershire. The particulars were communicated to that gentleman by John Jones, Esq., Vice-Consul at the port of Gloucester to his Majesty the Emperor of Austria :—

"Enclosed in a little box is a dried specimen of a small 'gar fish,' and a paper containing notes from the log-book of Captain Parsons, of the ship Harbinger, of the track and dates in which the fish was found on the passage between Mirimachi, New Brunswick, and the port of Gloucester. It was impossible, in the great distance through which he sailed, to pull up a ship's bucket without four or five dead gar fish. It appears the fish were most numerous in that latitude through which the volcanic band of Iceland, the Azores, the Canaries and Madeira just strikes, and I believe, therefore, that the immense shoals must have been destroyed by submarine volcanic action, and we may thus learn a lesson of the manner in which some of our fish-beds have been formed, and even of the destruction of genera and species."

The above short extract is of very great interest. The specimen of the fish itself, as nearly as can be made out from the state in which it was dried, is the *Sygnathus anguineus*—a species inhabiting the British seas, but having a considerable extent of range southward. Mr Yarrell informs me, he has seen specimens from the latitude of Madeira ; and this fact is of some importance, as it renders it more probable that the destruction was caused by submarine disturbance taking place within the zone to which Mr Symonds alludes.

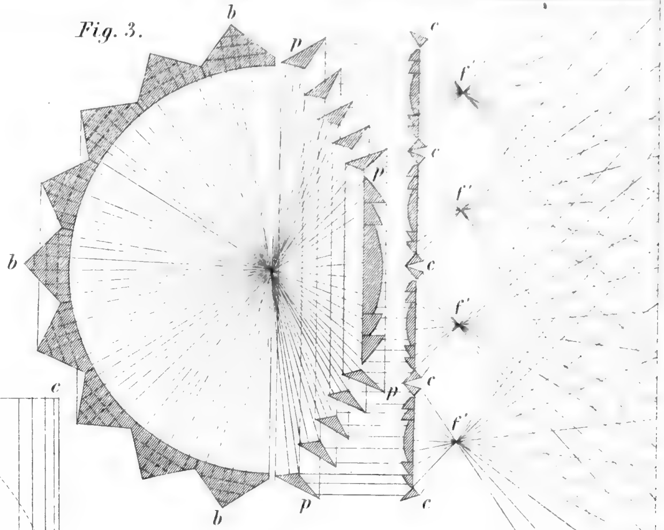
In the notice of the insular Volcano, Hotham island, which was raised in the Mediterranean, near the coast of Sicily, in 1831, "a great quantity of dead fish was observed floating in the sea the day before the island itself was discovered;" and any similar convulsion which tears open the bottom of the deep, and goes through all the phases of an active volcano, only submerged from human sight, must be fatal to all animal life in the vicinity; but the extent acted upon need not necessarily be so great, and the deadly bounds of the convulsion may take place within the limits of a few, and more especially within the particular habitat of some one species. That these submarine volcanic actions have been the cause of death to the species which form many of our fossil fish-beds is most likely, but it does not follow that these always took place in the vicinity of the present locality of the bed. Wherever the primary destruction of the Sygnathus occurred, it is not at all probable that it extended over nearly the whole range where they were seen by the captain of the Harbinger—it is much more likely that they were then being carried away by currents from the scene of the eruption, and we can easily conceive them so carried or drifted into some bay, or eddied into some hollow, and there deposited in mass; and the same causes would, ere long, cover them with a layer of sand or muddy silt, and place them in a modern fish-bed, far from the place of their destruction, and remote from the locality where the species was known to exist. Or if some shallow estuary happened to be the locality to which they were carried, and if they were left there during the ebb of one single tide, exposed to the sun and winds, the upper layer, at least, would be dried, bent, and crooked in every shape, their mouths open and their fins distended, and in such forms would they be sanded and silted over. We are not, therefore, in the case of fossil fishes, to consider that they always inhabited the localities in which they are now discovered. The Sygnathus was drifting over a range of many miles; its comparatively hard covering would permit it to stand immersion without decomposition for some time, and the state of its preservation, wherever it happened to rest or be laid, would be perfect, just according to the time of its exposure. It is remarkable that no other species was observed by

Fig. 1.



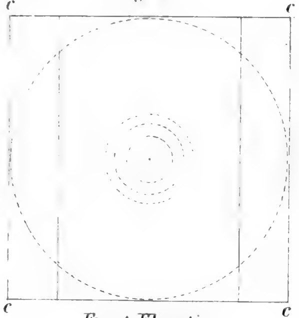
Middle Horizontal Section.

Fig. 3.



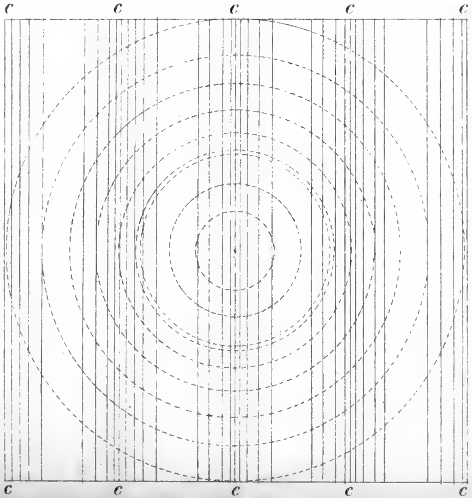
Middle Horizontal Section.

Fig. 2.



Front Elevation.

Fig. 4.

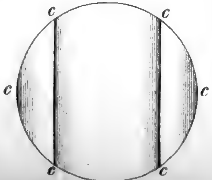


Front Elevation.

Fig. 5.

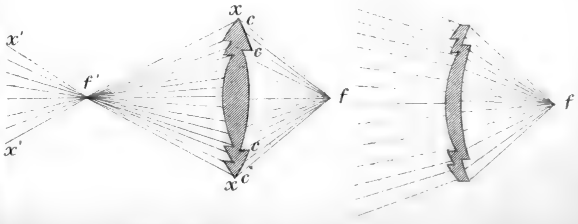
Outside Elevation.

Fig. 6.



Elevation next Flame.

Fig. 7. Fig. 8.



Horizontal Cross Sections.

W.H. Lizars Sc.

Captain Parsons, either dead or dying, through the long track in which he observed the "gar fish;" which we would account for either from the peculiar floating properties that would be possessed by a hard-skinned Sygnathus, or by the limitation of the range of the destroying agent; but although we cannot distinctly account for this circumstance, the fact is of interest as showing the occurrence of apparently similar causes in the destruction of the individuals forming the ancient fish-beds, which are sometimes filled almost with one species only.

The accompanying figures, taken from Captain Parsons' log, point out the track of the Harbinger in which the fish were seen.

LAT. N.	LONG. W.	LAT. N.	LONG. W.
	39° 0′	45° 56′	22° 0′
46° 49′	34 10	46 32	20 30
46 24	31 45	47 40	19 35
46 22	29 0	47 56	19 10
46 16	27 10	48 34	17 49
46 10	26 0	49 14	15 47
45 59	23 46	49 36	13 17

On a Simple Method of distributing naturally Diverging Rays of Light over any azimuthal angle, with description of proposed Spherico-Cylindric and Double-Cylindric Lenses, for use in Lighthouse Illumination. By THOMAS STEVENSON, F.R.S.E., Civil Engineer. (With a Plate.)

The diacatoptric apparatus of M. Augustin Fresnel is admirably adapted for the use of such fixed lights as require to be constantly visible in every azimuth. There are, however, many situations where only a small portion of the horizon requires to be constantly illuminated, and yet that portion may still be too great to admit of being lighted up by a single parabolic reflector, the divergence of which does not exceed 15°. In such cases the engineer has hitherto been contented, for want of more perfect apparatus, either to employ a segment of M. Fresnel's diacatoptric arrangement for fixed lights already referred to, or to have recourse to several parabolic reflectors, each of which requires its own separate lamp. When half the azimuth requires to be illuminated, the use of one half of the diacatoptric apparatus, having a spherical mirror behind, is perfectly legitimate, but where the arc to be illuminated is small, much light would obviously be lost. A better effect would probably result from using several parabolic reflectors, but these at first are costly, and, what is worse, the annual expense of maintaining so many independent burners is very great.

In order to supply this deficiency, I have already, in my description of the Holophotal system of illumination, published in the Transactions of the Royal Scottish Society of Arts for 1850, proposed a method of distributing all the diverging rays which proceed from a flame over 90° of the horizon. It is, however, often desirable to illuminate both larger and smaller arcs than the quadrant, and I now proceed to explain a simple method of attaining this end.

The whole diverging sphere of rays proceeding from the lamp is, in the first place, collected into one beam of parallel rays by means of a holophotal apparatus, and the rays so united are afterwards subjected to a second action, by which any desired amount of divergence may be produced.

In figure 1 (Plate IV.), *a* represents a parabolic conoid, truncated at its parameter ; *b* is a hemispherical mirror ; and *l*, a lens which, when placed at its proper focal distance from the flame, subtends the same angle from it as the outer lips of the paraboloid. The hemispherical reflector occupies the place of the parabolic conoid which has been cut off behind the parameter, and the flame is at once in the centre of the hemispherical mirror and in the common focus of the lens and paraboloid. Such an arrangement of optical agents constitutes a holophotal apparatus ; for if we suppose the whole sphere of rays emanating from the flame to be divided into two portions, namely, the hemisphere of front rays and the hemisphere of back rays, it is obvious that part of the exterior or front hemisphere will be intercepted by the lens, and made parallel by its action, while the remainder will be intercepted and rendered parallel by the paraboloid. The rays forming the posterior hemisphere, and which fall upon the hemispherical reflector, will be sent back through the focus in the same lines, but in opposite directions to those in which they came, whence, passing onwards, they will be in part refracted in a parallel direction by the lens, and the rest will be reflected in a parallel direction by the paraboloid. The back rays thus finally emerge horizontally in union with the rays from the anterior hemisphere.

This instrument, therefore, fulfils the condition of collecting the entire sphere of diverging rays into one parallel beam. Let *c c*, figs. 1 and 2, represent a series of straight prisms placed vertically and in front of the apparatus just described, and suppose their horizontal cross sections to be similar to those of the lens *l*, and their length to be equal to the greatest diameter of the paraboloid. It is obvious that the parallel rays impinging upon these prisms will be refracted in the horizontal, but will suffer no deviation in the vertical plane. The horizontal refraction will be similar to what takes place at the lens *l*, but will lie in the opposite direction, so that the rays will converge to a focus, f', in front, and will again diverge from that focus in the same angle in which they were made to converge towards it. An observer at any point in the azimuthal angle, G, f', H, will therefore have the benefit of a vertical strip of light whose height will be equal to the diameter of the paraboloid, *a a*, and whose width will be proportioned to the breadth of the flame employed. The principal objection to this arrangement is the inequality which must obviously exist in the intensity of the light as the observer passes from the middle of the azimuthal angle where the light will be brightest, towards the limits of that angle on either side where it will be weakest; for the lateral prisms do not intercept so large a portion of light as those which are nearer the centre of the beam of parallel rays.

In order to remove this objection, as well as to reduce the loss of light caused by absorption, I propose to use several sets of straight prisms, instead of having a single large one embracing the whole width of the holophotal apparatus. Such an arrangement is shown in figs. 3 and 4, in which is also represented the most perfect form of holophotal apparatus, in which totally reflecting prisms *p* and *b* are substituted for metallic reflection. The loss by absorption, which in metallic reflection amounts to about one-half of the whole incident light, is thus saved. It is unnecessary here to give a detailed description of this apparatus, as I have published it in th Transactions of the Royal Scottish Society of Arts, vol. iv., and as I have already in this paper fully described the nature of the holophotal action in the case where metallic reflection is employed.

Let us suppose, then, that the parallel rays proceeding from this apparatus are required to be deflected through a larger horizontal angle than in the former case, and that it is farther desirable to spread the rays more uniformly over the arc to be illuminated. Instead of one set of the straight refractors which are used in the former case, let there be four sets of such refractors, *c c*, figs. 3 and 4, each having two totally reflecting straight prisms, whose horizontal cross section is similar to that of the totally reflecting prisms *pp*, which are used in the holophotal apparatus of glass. Each set of refracting and totally reflecting straight prisms will have its own focus f' in front, from which the rays will diverge through the azimuthal angle due to the number of the prisms in each set, so that the denser, as well as the weaker portions of the light, will thus be destributed with sufficient uniformity over the whole arc. The same principle will also be found very useful for Apparent lights,* where the light near the confines of the illuminated arc has been found to become faint.

In order to save the loss of light due to the absorption and superficial reflection resulting from the use of two optical agents, I propose, in certain cases, to substitute for the lens commonly employed one on a new principle, having one side ground into the form of the straight refracting prisms *c*, *c*, *c*, *c*, shown at figs 5, 6, 7, while the other side remains convex, *but of different radius*. Fig. 5 shows the elevation of the outer surface of a lens on this principle, while fig. 6 shows the elevation of its inner surface, and fig. 7 its middle horizontal cross section. In the lens which I have represented in the figures, the rays contained in the larger conical angle $x, f, x,$ are spread over the smaller horizontal angle $x' f' x'$. The want of divergence which has often been complained of in the large polyzonal lenses in our revolving lights might easily be remedied by adopting this principle of construction. The convexity of the straight prisms in the horizontal plane would of course in such a case be very small. The bull's eye lenses used in hand lanterns, or in railway signal lights, might also with advantage be made on a similar principle.

---

* *Vide* Description of the Stornoway Apparent light, erected on a sunk rock in Stornoway Bay, the illumination of which is derived from a distant lamp situated on the shore.—*Trans. Roy. Scott. Soc. of Arts for* 1854.

It would, however, be better to grind one of the surfaces of the lens above described into *concave* cylindric *grooves* instead of convex cylindric prisms, thus making the lens of a *meniscal* section with one surface spherical, and the other cylindric, as shown in fig. 8.* *A meniscus lens, having its different rings cut out, or stepped on both sides,* would also be preferable to the plane convex form which is used in lighthouses, whether for fixed or revolving lights, as the thickness of the glass would be much decreased. My friend, Mr James M. Balfour, has suggested to me that good curves might also be obtained by placing the straight prisms *horizontally* on the outer face, thus making the *horizontal* section plano-convex, with such a curvature as to reduce the divergence to the required angle, while the vertical sections would be double convex, the outer curve being such as to render parallel the diverging rays as altered by the inner face.

A like effect might also be produced by making *double* cylindric a portion of one-half of the refractor which forms the middle compartment of Fresnel's fixed apparatus. Let us suppose a middle sector of this hoop of glass of such a number of degrees as the case may require, to be flanked on each side by the supplementary sectors, having their convex surfaces next the flame, and their outer surfaces ground into straight refracting and totally reflecting vertical prisms similar to those which I have already described for the outer face of the annular lens. The inner face of this refracting hoop will parallelize the rays in the vertical plane only, while the outer surface will produce either such an amount of direct divergence, or such an amount of convergence as will cause them ultimately to diverge through the desired angle. The same principle might be applied to the totally reflecting prisms when they form part of the apparatus, but the construction would probably be attended with too great difficulty.†

EDINBURGH, *Feb.* 12, 1855.

---

* Mr Adie has lately made for me lenses of the forms shown at figs. 7 and 8.

† The double cylindric refractor might be found serviceable for diminishing the divergence of the light in the Davy lamp, as well as for use in lighthouses. The effect would be increased by a back reflector of glazed earthenware or zinc, both of which materials I have found give a good light, and require almost no cleaning.

On Annelid Tracks in the Equivalents of the Millstone Grits in the South-west of the County of Clare. By ROBERT HARKNESS, Esq., F.R.S.E., F.G.S., Professor of Geology, Queen's College, Cork. (With a Plate.)

The existence of Annelida during the palæozoic formations is manifested in two conditions. In the one, we have the shelly envelope which invests the order Tubicola, in the form of serpolites; and in the other, the tracks of the orders Abranchia and Dorsi-branchiata are found impressed on deposits which were, at one time, in a sufficiently soft state to receive the impressions of the wanderings of these animals.

Among the strata which have hitherto afforded annelid tracks, those which, in the county of Clare, represent a portion of the equivalents of the millstone grit, contain such tracks, in their most perfect state of preservation in great abundance; and these strata also furnish evidence concerning the circumstances which prevailed during their deposition.

The locality of these strata is the neighbourhood of Kilrush, on the banks of the Shannon, in the southern portion of the county. Here the deposits consist of strata which have a flaggy character; these have been extensively wrought at Money Point, about four miles east from Kilrush, and they supply the flags which are commonly used in the towns of the south of Ireland. The beds vary somewhat in their nature, and with this circumstance they present different phenomena.

The higher portion of the deposits contain flaggy beds of a light gray colour, having sometimes a slight green tinge. These flags are thin-bedded, and although devoid of annelid tracks, they are marked with impressions of plants, principally calamites, in compressed fragments.

These higher flags also, in some instances, afford very beautiful ripple-markings, which are frequently so perfect as to enable us to judge from them of the direction of the wind from whence they resulted; and this appears, for the most part, to have been from the south, the larger slopes of the ripples pointing in that direction, the more perpendicular sides being towards the north.

R.K.G. delin.

Lizars, Litho.

TRACKS OF THE NEREITES CARBONARIUS.

These greenish-gray flags, as they pass downwards, appear to lose the remains of plants, but in the place of them we have the annelid tracks, at first rather sparingly, but at a slight distance below these seem to occur in considerable abundance. We have the greatest amount of these tracks in the lower beds of the quarry, and here they present a different aspect, the nature of the flags and their colour also varying from the higher portions of the strata; and the most abundant occurrence of these impressions is in the strata which have a dark colour, and these dark-coloured flags constitute the portion which is wrought for commercial purposes, the higher and lighter-coloured flags being rejected as rubbish. With these dark-coloured flags there occur intercalated beds which are devoid of the flaggy nature, in a great measure, not being easily divided along the laminæ of bedding; and these are regarded as building stones. The latter prevail most abundantly in the lower portion of the flaggy strata, and they gradually assume a more important position, until they form exclusively the lower portion of the more solid strata as here exposed.

The inclination of the strata which form the deposits at this locality is towards the north, at an angle of 15°. Beds of a similar nature seem to form the whole south-west portion of the county of Clare; and we have them well developed at Kilkee, about nine miles north-west from Kilrush. Here they are also worked for flags and building-stones, and they present the same lithological features, and afford the same fragments of plants, as well as tracks of annelids, as occur at Money Point; and in every respect they appear identical, having the same angle of inclination and direction of dip at the quarry where they are wrought.

This, however, so far as respects the latter, appears only in local circumstances, since we find the same deposits, which are well seen in the cliffs of this coast, in the neighbourhood of Kilkee, having various inclinations and directions.

Referring more particularly to the annelid tracks, these occur in three conditions. When they are in their most perfect state, in the faces of the higher greenish-gray flags, they have the form of meandering tracks, about half an inch across, and their margins crenated. A distinct raised line traverses the centre of these tracks, and the interval between this line and the crenations is marked by a succession of other lines at right angles to the centre one; and these seem to have had their origin in the rings of the body of the annelid. This being the appearance usually presented by the upper side of the flags, the under side of which affords natural casts, the tracks appearing in relief. This perfect state of the tracks does not prevail to the exclusion of more imperfect ones, for these perfect tracks manifest themselves only to a small extent, and can be gradually traced, losing their perfection, until the crenations and the central line disappear, the track assuming the form of a slightly depressed sinuous line.

From the nature of these tracks, when most perfect, we have evidence of some features in the structure of the animals from whence they originated. The outer crenated margin resulted from the organs of locomotion of these animals, the *cirri*, and we may consequently infer that these annalids appertained to the cirrated tribe rather than to the order of Abranchia, as this is represented by the present genus *Nais*.

The transverse lines which cover the impressions point out the annelid structure, and at once show that these impressions have not arisen from the wandering of molluscs, the tracks of which are sometimes seen on the surfaces of the sandstone strata; and the central line results from the ventral arch, which in this class runs along the central portion of the lower side.

The nature of the tracks, as they occur in the lower dark-coloured flags is somewhat different. On the upper surfaces of these they appear also in the form of sinuous furrows, about the same width as the more perfect tracks of the higher flags. Here, however, they rarely present crenations, being regular on their margin, and having, in many instances, the impression of the ventral arch distinct. In these lower dark-coloured flags traces of annelids are not confined to the surfaces of the strata exclusively. The inner parts of these flags very often furnish traces of annelids to a greater extent than even the surfaces of the beds themselves; and these too are usually marked by such circumstances as support the inference that they have sprung from somewhat different circumstances. When these flags are divided along the laminæ of bedding, the lower surface often presents the aspect usually afforded by the higher natural surfaces of these dark-coloured flags, having the sinuous line, with traces of the ventral arch. The surfaces of these sinuous lines, as they are exposed by the cleaving of the flags, are in general much finer, and we commonly find that they are filled with the substance of the stone, appearing in the form of a thick worm, a transverse section of which has a particular shape, and on applying the split surfaces to each other, it can be seen that these apparent tracks are really the burrows of the annelids in the stone, when it was originally mud, and not tracks on the surface of the sea-bottom. These ancient burrows bear every evidence of having been lined with mucus, as are the recent burrows of this tribe of animals; and into these burrows the mud has flowed, filling them up, and now furnishing the worm-like bodies which are exposed on the split surfaces of the flags.

These worm-shaped bodies give to us the shape of the burrow, which was meandering, and flattened perpendicularly.

There is another circumstance which the surfaces of the higher greenish-gray flaggy strata sometimes presents, which consists of a small funnel-shaped cavity, forming the entrances into these burrows.

The various appearances of the tracks, and the nature of the strata with which these are associated, furnish some important information concerning the conditions which obtained when this portion of the millstone grit series was being deposited. With regard to the tracks themselves, these, from their various states of perfection, indicate that, in some instances, the mud which now constitutes these flags had been in different states, as concerns consolidation, at the time when it was traversed by these animals. It sometimes appears to have been in a state so saturated with water that it assumed a pasty condition, partly flowing in upon the tracks after these had impressed its surface, and obliterating the markings of the cirri. At other times it seems to have been sufficiently consolidated to afford the requisite conditions for more perfect tracks, as in the case of the higher greenish-gray flags. But even here, we often, as already stated, see these more perfect tracks becoming less distinct, leading to the inference, that while some portions of the bottom of the sea, occupied by this lighter mud were comparatively hard, others were so soft as to flow in and in part efface the impressions. This is a circumstance which occurs not only with annelid tracks, but likewise with the impressions of reptilian footprints, as these are seen on some of the faces of the Bunter sandstone strata. In this latter case, although the conditions under which footprints were formed were different, being generally the result of the wanderings of reptiles on a sandy shore; still the cause of their partial obliteration was the same, namely, the flowing of sand saturated with water into the impressions after the foot had been removed.

The occurrence of annelid tracks is not confined to the flagstones of the county of Clare, although these possess characters which are the most perfect and beautiful of any which have hitherto been discovered. They are met with in many of the palæozoic rocks, and frequently under the same conditions in which they occur in this locality, namely, on the surfaces of the flagstones. Sir Roderick Murchison, in the Silurian system, figures a track from the schistose building stone of Llampeter, which he calls *Nerites Sedgwicki*, and this has a great resemblance to the impressions on the flags of the county of Clare. Markings of a similar nature also occur among the lower Silurians of the south of Scotland, and have been named Crossopodia by Professor M'Coy, and described by him in the account of the fossils added by Professor Sedgwick to the Woodwardian Museum. Tracks somewhat resembling those of the county of Clare, both in nature and age, are mentioned also by Mr Binney, as being present in the flagstones of Hutton roof, near Lancaster; and these are described by him in the Transactions of the Literary and Philosophical Society of Manchester, vol. x., p. 189. This geologist also mentions annelid markings which are found in the form of holes in the flags of the lower portion of the Lancashire coal-field; and the upper portion of these holes seem to have a great affinity to the funnel-shaped cavities which the entrances into the burrows of the annelids present, as they are seen on the surfaces of the Money Point flags, and these holes, Mr Binney regards as having resulted from the Dorsi-branchiat annelid, to which he applies the name of *Arenicola carbonaria*, considering it as the ancient representative of the Arenicola piscatorum, or lug-worm of our present sandy shores. The Lancashire markings do not afford the crenations which accompany the tracks on the flags of the county of Clare, and the deposit in which they occur is of a somewhat different mineral nature, having more of a sandy character than the equivalents of the millstone grits as they are represented by the deposits of Money Point and Kilkee, where the beds seem to have been originally of a more muddy nature, and probably where a different habitat prevailed.

The animals which impressed these Irish flags appears to have been widely different from those which have burrowed in the deposits which now form the flags of the lower portion of the Lancashire coal-field, since, in these latter, neither the entrance into the burrows nor the burrows themselves, equal the annelid burrows of the flagstone of Clare; the former having only a diameter of ⅕th of an inch, and being apparently round, while the latter are ½ an inch in breadth, and have their form flattened longitudinally, which gives to them on transverse section, the lenticular shape already referred to. From their crenulated margins, which would indicate that the cirri were more perfectly developed in the annelids to which we owe these tracks, it would seem that they are more nearly allied to those which have impressed the strata of the older formations, than to such as have left their markings on the English carboniferous deposits; and if we adopt the generic appellation of Sir Roderick Murchison, they might be considered as the carboniferous type of the ancient *Nerites,* and be designated *Nerites carbonarius.*

Nerites carbonarius (Harkness), *Plate* V.

Tracks, when in their most perfect state, sinuous, about ½ an inch in breadth, and having a central line running along them, the result of the ventral arch. The sides of the tracks crenulated, the effect of the *cirri* which seem to have been largely developed. Where the burrows are seen, these appear in the form of sinuous hollows which have been filled up with mud, and these hollows which were originally lined with mucus, present, on transverse section, a lenticular form, and also show on their lower side the raised central line produced by the ventral arch.

Locality.—Occur in great abundance, and in a very perfect state, in the flaggy equivalents of the millstone grits at Money Point, and at Kilkee in the south-west of the county of Clare.

Description of New Coniferous Trees from California. By ANDREW MURRAY, W.S.

The expedition, in the course of which the trees now to be described were found, was undertaken by my brother, Mr William Murray, last autumn. He was joined by Mr A. F. Beardsley, a gentleman from whose energy and knowledge of the mode of life in the regions they traversed, he derived much assistance. They left San Francisco in the month of September, and directed their course northwards and eastwards, so as to explore the country lying between the coast range and the Rocky Mountains. For a great distance their researches were not rewarded by the discovery of anything of much interest or novelty, and they were almost despairing of success, when they came upon one of those patches of country so characteristic of North-western America, in which were crowded together a number of totally new species, as well as several of the rarest of those which have been already described or introduced into this country.

Among the pines they found and procured seeds of *Abies nobilis* and *grandis*, *Pinus Jeffreyi*, *monticola*, *Benthamiana*, *tuberculata*, *Lambertiana*, &c. Whilst camped amongst these their attention was a good deal directed to their growth and habits, and some of the information which they acquired regarding them might be practically useful to the cultivators of them in this country. For instance, the difficulty of procuring sound seed of *Abies nobilis* and *Abies grandis* is well known. No collector who has met with it (and a number have gone expressly to secure it) has omitted to send it home; but (with the exception perhaps of the seed sent by Douglas) I believe it has invariably arrived so bored and worm-eaten by maggots, that it has germinated only in rare instances. The

Fig. 1.

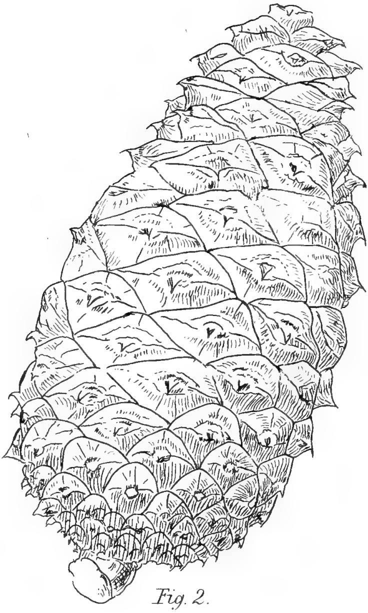


Fig. 2.

Fig. 5.

Fig. 4.

Fig. 3.

P. Beardsleyi.

A Murray Delt. et Sculpt.

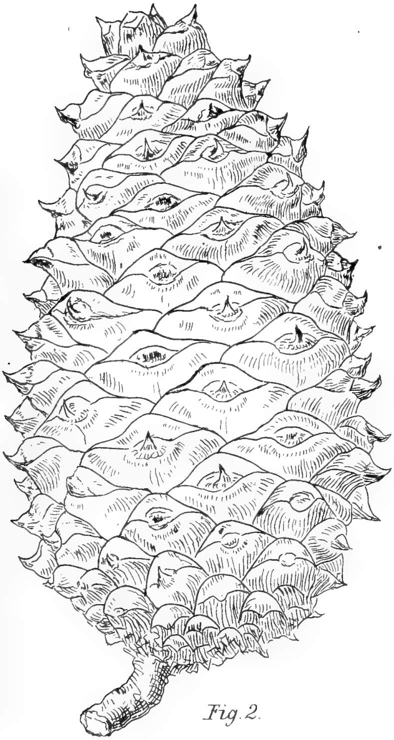


Fig. 2.

Fig. 1.

Fig. 5.

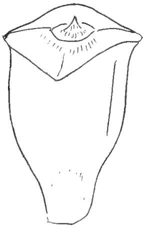


Fig. 4.

Fig. 3.

P. Craigana.

A. Murr [illegible] Sculp.t

PLATE VIII.

Edinburgh New Philosophical Journal Vol I.

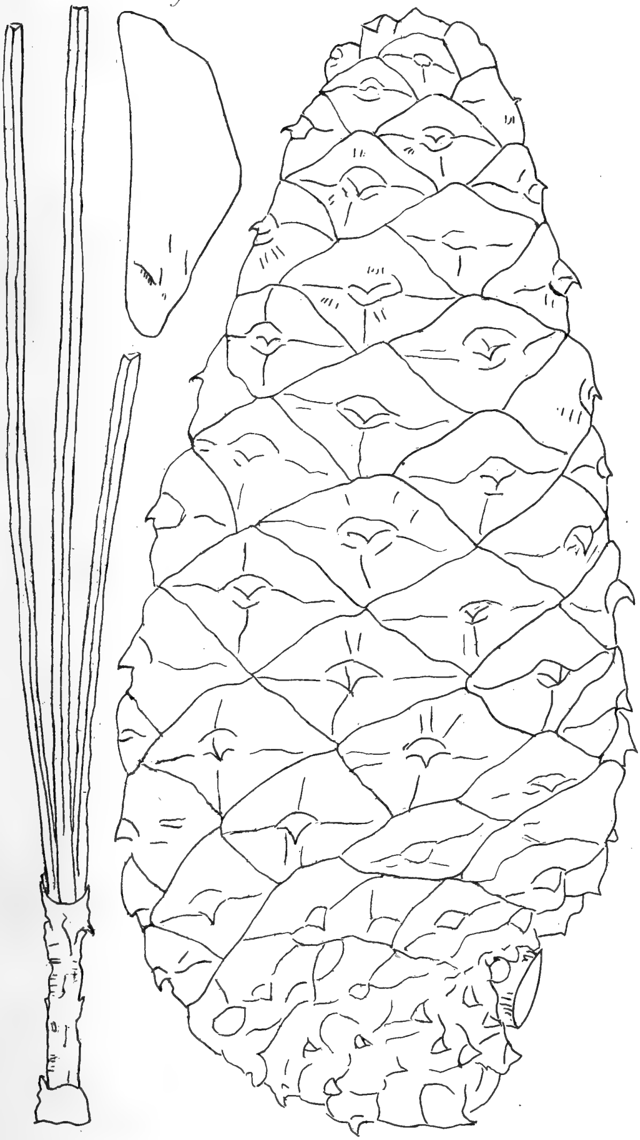


Fig. 3.

Fig. 1.

Fig. 2.

P. Benthamiana.

(Copied from Hartweg)

A. Murray, Sculp.t

PLATE IX. *Edinburgh New Philosophical Journal* Vol I.

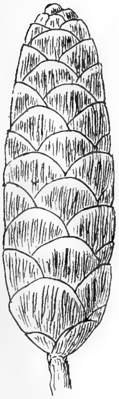


Fig. 13.

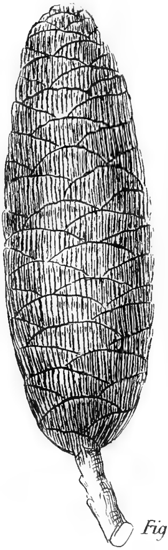


Fig. 3.


Fig. 12.

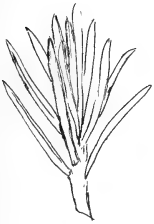


Fig. 11.


Fig. 1.


Fig 2.


Fig. 14.

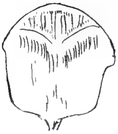


Fig. 15.

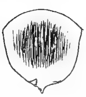


Fig. 4.


Fig. 5.

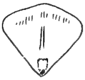


Fig. 8.


Fig. 17.


Fig. 16.


Fig. 10.


Fig. 9.


Fig. 7.


Fig. 6.

A. Hookeriana *A. alba.* *A. Pattoniana.*

A. Murray, delt. et sculpt.

PLATE X.

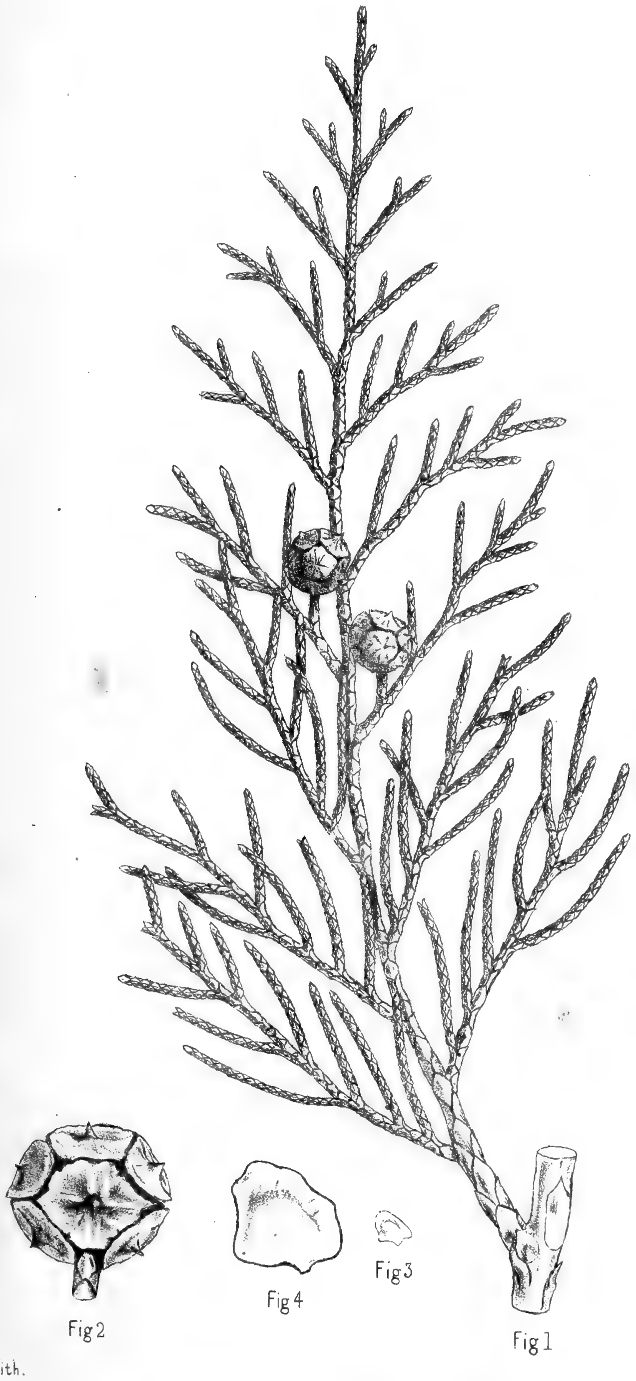


Fig 2 Fig 4 Fig 3 Fig 1

A. Murray, lith.

CUPRESSUS LAWSONIANA

R. K. Greville del^{t}

A. Murray. Sculp^{t} et Lith^{r}

CUPRESSUS MACNABIANA.

supply of these trees, therefore, continues to be dependent upon grafts and cuttings, of course at a high price. Various causes have been alleged as the ground of these failures, the most common suggestion being the carelessness of the collector in not selecting perfectly sound seed, and want of care and attention in packing. My brother's observations have satisfied him that to neither of these can previous failures be justly attributed. They are entirely owing to the cones of the trees being so universally attacked by an insect that it is matter of the greatest difficulty to find an untouched cone. It was only after examining the produce of hundreds that my brother was able to secure a very few in tolerable condition. At no period of their growth did he find the cones free from it. The insect appears to lay its egg in the seed while the cone is still in its green and tender state. Probably it could not penetrate the hard husk of the seed in its mature state, and in the majority of cones almost every seed will be found with a maggot in the kernel while it is still unripe. What species of insect it is, we cannot yet tell. The grub is obviously the larva of a coleopterous insect, and I hope shortly to know more, as I am at present in process of attempting to breed some living specimens which my brother brought home. He found no perfect insect in or among the cones, with the exception of a single specimen of an *Agathidium*, which he shook out of a heap of cones. Neither the larva of this minute beetle nor its habits are yet known, so that we cannot say positively whether it is the culprit here or not; but so far as an inference may be drawn from the known habits of the perfect insect, which frequents decaying vegetable matter, I should say it is not. From the appearance of the larva I think it is more likely to be an *Anobium*, in partial confirmation of which I may mention that in one of the consignments sent home by Jeffrey, there was found among the debris a considerable number of living specimens of an *Anobium* closely allied to our *Anobium molle* and *Abietis*, both of which feed upon some portion of our fir-trees. I cannot charge my memory whether these specimens came in the consignment of which seeds of the *A. nobilis* formed a part or not. Whatever be the species, it is obvious that it must be found in immense numbers at the season when the perfect insect is eclosed. It also appears to be widely distributed, having been found so universally, from whatever locality the seed may have been taken; so that, unless some fortunate season occurs unfavourable to the existence of this pest, it appears probable that these magnificent and beautiful species are likely to continue scarce and valuable in this country, the small quantity of good seed likely to be procured by a collector not being sufficient to repay the labour and expense of procuring it.

But the discussion of such points relating to trees already established in this country is foreign to our present purpose, which is simply to describe one or two new species which were found associated with these pines.

The first of these is a species of *Pinus*, which we have named *Beardsleyi*, in honour of Mr Beardsley.

Pinus Beardsleyi. Plate VI.

P. foliis ternis, longis; vaginis curtis, corrugatis; strobilis, oblongis equilateri-ovatis, aggregatis; squamis apice quadrangulis, umbilico mediocri; elevato mucronatis; mucrone tenui versus basim deflexo; spermodermate maculato.

Habitat in California in montibus interioribus circa lat. 41° *Bor.; altitudine* 5000–6000 *ped.*

Leaves in threes, about six inches long, firm, numerous, roughened by projecting points along the midrib and edges, the points directed to the tip of the leaf. *Sheath* short, about an eighth of an inch in length, coarse and corrugated. *Cones* growing on a short thick peduncle, aggregated round the branch, generally from 3 to 5; 3 inches long, and 1½ across, or nearly 5 in circumference at the broadest part, of a somewhat prolonged elliptical shape; and the difference in the appearance of the scales on the outer and inner side of the cone is trifling. *Scales* an inch long, with a not very prominent apophysis. The medial line, crossing the exposed part of the scale, generally runs nearly across the middle. A thin small sharp hooked prickle, nearly a line in length, points towards the base of the cone. *Seeds* winged with a speckled spermoderm, about 1½ lines in length, wing 7-8ths of an inch in length, pale brown, semitransparent, darker at the tip, and with brown streaks running longitudinally.

The tree is of great beauty and size; one which was cut down measured 123 feet in height, and 44 inches in diameter at the stump. Another tree near it measured 17 feet 4 inches in circumference at three feet from the ground. The stem was a very handsome column about 30 feet to the first branch; timber good and clear. It was found on the top of a mountain, in lat. 41° N., at the same altitude as *Pinus Jeffreyi* and *monticola*, and *Abies grandis*, and higher than *P. Benthamiana* and *Lambertiana*.

This and the following species (*Craigana*) seem to have more affinity with *P. Benthamiana* than any other described species. But the present species has the points of the umbo of the scale pointing towards the base of the cone, while in *Benthamiana* they point to the tip; the cone of *Benthamiana* is 5 inches long, while *Beardsleyi* is only 3 inches. The leaves are 11 inches in length, while in *Beardsleyi* they are only 6. The sheath of the leaf in *Benthamiana* is an inch long, while in *Beardsleyi* it is only an eighth of an inch. The wing of the seed of *Benthamiana* is much larger and longer than that of *Beardsleyi*. The timber of *Beardsleyi* is homogeneous all through. The heart of *Benthamiana* is redder than the sap wood, and the sap wood occupies a great breadth of the stem. *Beardsleyi* grows much further up the mountains than *Benthamiana*. The distinction between the cones of these trees will be sufficiently seen from the rough etchings which I have given. The figure of the cone of *Benthamiana* is copied from that given by Hartweg. Like all that gentleman's figures and descriptions, it is very characteristic of the cone as it is generally found, but it is inaccurate as a representation of the cone in its complete state, in so far that it represents the hooked umbo as pointing to the base. In point of fact it does take a bend in that direction, but the prickle which terminates the umbo takes a sudden turn backwards, and points to the tip like the following species (*Craigana*). The prickle in the specimen, from which Hartweg's figure has been taken, has obviously been rubbed off, which gives a false impression of the direction of the umbo. There can be no doubt about this, because my brother found all Hartweg's localities so strictly correct that he could recognise the very patches of different trees that he describes having met; and he took his observations on the cones, &c. of *Benthamiana* from the very clump of that tree described by Hartweg, as found by him near Santa Cruz. There was no other tree, or clump of trees, for a great distance, with which it could be confounded.

There is also some resemblance between this Pine (*Beardsleyi*) and *P. ponderosa*, as was well suggested to me by Dr Lindley; but the shape of the cone, and the size and shape of the seed and wing sufficiently distinguish it. In *P. ponderosa* the cone tapers to both ends, while in this it tapers to the point. Its seed does not appear to be speckled in any figure I have seen (I have not seen any specimen of the seed itself), while this is. The sheath of the leaf in *P. ponderosa* is smooth longish, fine, and tightly fitting, whereas in this it is short, corrugated and rough; and the leaf of *ponderosa* is nearly twice as long, being 9 to 11 inches in length, in place of 6 inches. Its leaf also wants (or nearly so) the projecting points which roughen that of *Beardsleyi*, so that the leaves can be distinguished by the feel, or drawing them forwards between the fingers.

Pinus Craigana. Plate VII.

P. foliis ternis, delicatis; vaginis longis teneris; strobilis fere equilateri-ovatis pedunculatis aggregatis; squamis apice quadrangulis, umbilico mediocri elevato mucronatis, mucrone versus apicem spectante, spermodermate maculato.

Habitat in California, circa lat. 41° *Bor., altitudine* 4000–5000 *ped.*

Leaves in threes, 4¼ inches long, thin and fine. *Sheath* ⅓d inch long, fine, smooth, and tightly fitting. *Cones* light brown, 3 to 3½ inches long, and nearly 2 inches across, and about 6 inches in circumference at the broadest part; oblong elliptical. There is a little difference between the scales on the outer and inner side, those on the outer being rather more developed, but it is not very marked. *Scales* an inch long, with a strongly-marked apophysis. The medial line crosses the exposed part of the scale within a third of the top. A pretty strong short-hooked *umbo*, after making a short curve towards the base, points to the tip of the cone. *Seeds* winged, $\frac{1}{8}$th of an inch in length. *Spermoderm* speckled. *Wing* $\frac{5}{8}$ths of an inch in length, and nearly $\frac{3}{8}$ths of an inch across, pale fawn-coloured, darker at the tip, and with purplish-brown streaks running longitudinally. There is a small, rounded, purple-tipped bract, $\frac{1}{8}$th of an inch in length, at the base and back of the scale.

It differs from the preceding species (*P. Beardsleyi*) in having the prickle of the scale pointing towards the tip instead of the base. The prickle, too, is strong and firm in *Craigana;* in *Beardsleyi* it is small and weak. The apophysis, or excrescence on the exposed part of the scale, is smaller in point of space, but more prominent in *Craigana* than in *Beardsleyi*, which has the exposed part somewhat flat, while in *Craigana* the upper part projects over the lower. The wing of the seed of *Craigana* is shorter, and relatively broader. The seed is nearly twice the size of that of *Beardsleyi*, although the cones are about the same size. The leaf of *Craigana* is finer than that of *Beardsleyi*, and not so long. The sheath of the leaf is finer, and considerably longer.

Craigana was found on the same mountains as *Beardsleyi*, but growing lower down, and below it again appeared *Benthamiana*. It spreads its branches, wider from the stem than *Benthamiana*, and sheds its seed a month later.

My brother and I have dedicated this handsome pine to Sir William Gibson-Craig, Bart., whose enthusiasm has done so much to promote the cultivation and introduction of new pine trees, and who, in particular, was one of those who chiefly conduced to my brother undertaking the expedition, of which this pine forms part of the fruits.

Abies Hookeriana. Plate VIII.

A. foliis curtis, utrinque concoloribus; strobilis cylindricis, pallide-fulvis, bracteolis tenuibus minutis ad basim squamarum stricte applicatis ; squamis concavis et non crenulatis.

Habitat in California, in iisdem montibus quam precedens, sed majore altitudine.

Leaves slightly curved, with a rib in the middle, both above and below, and sometimes depressed above, so as to give the leaf a triangular or boat-shaped form; from $\frac{1}{2}$ to $\frac{3}{4}$ths of an inch long, not silvery beneath. They are closely but irregularly set along the young branches, chiefly on the upper side of the branch, except at the extreme shoot, where they closely surround the whole twig. The general appearance of the foliage is crowded. *Cones* cylindrical, oblong, from $1\frac{3}{4}$ to 2 inches long, and $\frac{1}{4}$th of an inch broad, pale fawn-coloured. *Scales* somewhat concave or saucer-shaped, dull and opaque, more especially where they have been covered by the other scales, slightly thickened at the exposed edge, not crenulated, but gently impressed with two or three faint raised lines; these lines, irregular and evanescent, generally running straight down the exposed part of the scale, or only sloping slightly towards the centre. Sides of the scale cut out unequally on the opposite sides, and ending with a tooth curving inwardly at each side of the root. A small bract is situated at the bottom of each scale, fastened firmly to the back, and adpressed upon the scale. It is nearly two lines long. There is a yellowish tooth in the middle, which is a mere prolongation of the nerve by which it is attached to the scale, and which is so firmly fixed that the scale may be torn off, leaving the greater part of the nerve sticking like a thread to the scale. The top, on each side of this scale, is purple. At about one-third of its length from the top the breadth of the bract is suddenly contracted and from thence slopes gradually to its root.

This species is allied to *A. alba.* The cones have considerable resemblance. They are of the same colour, and the scales in both are somewhat saucer-shaped, and have their edges smooth; but *Hookeriana* has the cone, and more especially the scale, seed and wing larger. These, as well as the bract at the back of the scale, are differently shaped, as will be seen from the figures in the etching. The habit of the tree, and the manner of growth of the leaves, is also different. In *A. alba* the leaves are inserted pretty regularly along the branch. In *Hookeriana* they are crowded together, curling upwards a little, after the fashion of *A. nobilis.*

This *Abies* has also considerable resemblance to *A. Patton-*

iana, introduced three or four years ago by Jeffrey, the collector sent out by the Edinburgh Oregon Expedition, and as that species is little known (having only been described and figured in a private circular issued by that Association), I shall enter a little more at length into the distinctions between the two than I have done with *A. alba.*

Both *A. Pattoniana* and *A. Hookeriana* are trees of exceeding beauty, but the former is described by Jeffrey as being 150 feet in height, and towering over the rest of the forest. The height of *A. Hookeriana* was only about 50 feet. One tree that my brother cut down measured 47½ feet in height, and was 20 inches in diameter at the stump. The timber is hard and tough. It is more distinguished by its gracefulness than its size. With the exception of *Cupressus Lawsoniana* (to be presently mentioned), my brother describes this as the most beautiful of the new discoveries which his expedition produced. Its gracefulness and elegance were the qualities on which he particularly dwelt. The cones of the two trees give many points by which to distinguish them. They do not differ much in size, but those of *A. Pattoniana* are of a dark brown colour, and those of *A. Hookeriana* of a light fawn colour, somewhat of the hue of the cone of our common larch, or of *Abies alba.* The scales of *A. Pattoniana* are a third, or a half smaller than *A. Hookeriana.* They are deeply crenulated quite down to the place which the bract covers, and that place is smooth and prominent. The scales of Hookeriana are not crenulated, an evanescent raised line only shows itself here and there. The shape of its scale also is not regular; it is cut out on each side, but one side is always more cut out than the other; where the cutting out has commenced, the scale has thinned off so as to be membranaceous. In *A. Pattoniana* there is no such thinning off nor cutting out. In its scale the place where the two next scales have lain over it is not, or at least is scarcely, to be distinguished from the exposed part. In *A. Hookeriana* it is very marked, there being an immediate rising or thickening in the line of the scale just beyond where they lay, showing the exposed part very distinctly of a curved triangular shape. The surface of the covered part in *A. Hooker-*

iana is duller and more opaque than the exposed part, and the streaks or raised lines are less perceptible. In *A. Pattoniana* no such difference exists. The bract in *A. Pattoniana* contracts at about two-thirds of its length from the top, and has a projecting purple ear immediately before the contraction. *A. Hookeriana* has no such ear, and the contraction takes place at one-third from the top instead of two-thirds. This ear is not to be confounded with a sort of projection, which both have at the top angles. The seed and the wing of *A. Pattoniana* are both about one-third shorter than in *A. Hookeriana*, and the wing of the former has a purplish-brown tinge at the top and back, which does not exist in the latter.

This species was found high up the Californian mountains, about lat. 41° N., where the ground was already covered with snow, on the 16th of October.

We have named this species in honour of Sir W. Hooker, who has done so much for the botany of this country. The species *A. Pattoniana* was justly named by the committee of the Oregon Botanical Association, after Mr Patton of the Cairnies, in Perthshire, a gentleman who is following out with equal zeal and discrimination a series of experiments, having for their object the ascertainment of what new pine and other forest trees can be grown with most advantage in our climate.

Cupressus Lawsoniana. Plate IX.

C. ramulis quadrangulis mediocriter compressis flexuosis; foliis crassis decussatis quadriseriatis adpressis; strobilis polygonis pedunculatis; squamis fere planis, mucronatis; seminibus planis, auriculatis.

Habitat in California, in lat. 40° *ad* 42° *Bor.*

Branchlets quadrangular, somewhat compressed; *leaves* decussate, ovate, glaucous, adpressed in four imbricated rows; *cones* polygonal, of a light brown colour, about the size of a large pea, pedunculated; scales, six in number, flat, rough, light brown, corticaceous, irregularly four or five sided, with an umbo or tooth in the centre, pointing straight outwards. *Seeds* proportionally large, flat, somewhat ear-shaped. *Branches* flexuose, crowded, ascending.

This was the handsomest tree seen in the whole Expedition.

It was found on the banks of a stream in a valley in the mountains; is about 100 feet high, and two feet in diameter. The foliage is most delicate and graceful. The branches bend upwards at the end like a spruce, and hang down at the tip like an ostrich feather. The top shoot droops like a Deodar. The timber is good, clear, and workable.

This species has been named after Messrs Lawson, the enterprising nurserymen of the Scottish capital, who, after having distributed and made generally known so many species of this family of trees, are well entitled to have their names connected with a species likely to prove a general favourite; and the attention comes well from my brother, who, if he has received praise and commendation from others for the extent and excellence of his collection, has received from these gentlemen the solid pudding, they having purchased the whole of his collection at a liberal price.

Cupressus M'Nabiana. Plate X.

C. foliis acutis, carinatis, decussatis; ramulis curtis tortuosis; strobilis globosis, squamis mucronatis, mucrone contorto; seminibus parvis, concavis.

Habitat in California, circa lat. 41° *Bor.*

Leaves acute, keeled, decussate, sub-amplexicaul at the base, the older leaves ending in short firm projections. *Branchlets* small, short, tortuose. *Cones* globose, growing on a short thick peduncle, about the size of a small cherry or gean. *Scales*, six in number, irregularly four or five sided, each with a strong projecting mucro in their centre, the mucro generally curled in at the point, especially in the younger cones. *Seeds* small, circular, bent in the shape of a scoop. The figure given of this species is taken from a dried specimen, from which many of the leaves and branchlets had been broken off, so that it appears less clothed than it is in reality.

An evergreen shrub, growing to no great size, but from its somewhat gnarled and tortuous appearance, likely to form an agreeable variety in a lawn or shrubbery.

My brother has named this species after our friend Mr M'Nab, of the Royal Botanic Garden, Edinburgh, who contributed much to the success of the Expedition by his judicious advice and suggestions, particularly as to the best mode of safely transmitting the seeds to this country, advice which proved not only eminently practical, but also singularly successful.

Taxus Lindleyana.

T. foliis bi-seriatis, linearibus, planis sparsis ; baccis ut in Taxo baccata hibernica ; seminibus fere globosis ; ramis longissimis et pendulis.

Habitat in California, circa lat. 40° *ad* 41° *Bor.*

Leaves two-ranked, linear, flat, of smaller size and narrower than in the common British yew (*T. baccata*, L.), and the prickle at the end of the leaf is more developed. *Berries* exactly like those of the Irish yew, growing on the under-side of the branch. *Seeds* nearly globose, putty-coloured. *Branches* exceedingly long and pendulous. *Wood* almost as elastic as whalebone—a property which has been turned to useful account by the Indians, who make their bows of it.

As I have only an imperfect specimen of the branch and seed, I am sorry that I cannot give more than the above very meagre description.

The tree is from 40 to 30 feet high. One which my brother measured was 50 inches in circumference at 5 feet from the ground. Another at the same height measured 5 feet 10 inches in circumference. It was found growing on the sides of a glen under the shade of larger trees which grow higher up. It would consequently make a good filler-up where ordinary underwood does not readily grow.

I have named it after Dr Lindley, whose courtesy and kindness, both now and formerly, in examining for me, and reporting upon specimens sent from abroad, I take this opportunity of gratefully acknowledging.

It is perhaps unnecessary for me to say, that in the foregoing paper, all the information (other than which I could acquire from the actual inspection of the specimens themselves), was obtained from my brother ; and I regret that a more engrossing occupation should have at present prevented him from describing these interesting novelties himself, a regret which should be participated in by the reader, as had he undertaken it, the work would have been much better done.

Explanation of Plates.

Plate VI. *Pinus Beardsleyi.*
  Fig. 1. Leaves of do.
  2. Cone.
  3. Bract of Scale.
  4. Scale.
  5. Seed.

VII. *Pinus Craigana.*
  Fig. 1. Leaves of do.
  2. Cone.
  3. Bract of Scale.
  4. Scale.
  5. Seed.

VIII. *Pinus Benthamiana* (copied from Hartweg's figure).
  Fig. 1. Part of Leaves.
  2. Cone.
  3. Seed.

IX. Fig. 1. Twig of *Abies Pattoniana.*
  2. Leaf of do.
  3. Cone of do.
  4. Inner side of Scale of do.
  5. Outer side of do.
  6. Bract of do.
  7. Seed of do.
  8. Outer side of Scale of *Abies alba.*
  9. Bract of do.
  10. Seed of do.
  11. Twig of *Abies Hookeriana.*
  12. Leaf of do.
  13. Cone of do.
  14. Inner side of Scale of do.
  15. Outer side of Scale of do.
  16. Bract of do.
  17. Seed of do.

X. *Cupressus Lawsoniana.*
  Fig. 1. Twig of do., with Cones.
  2. Cone of do.
  3. Seed of do. (natural size).
  4. Do. (magnified).

XI. *Cupressus M'Nabiana.*
  Fig. 1. Branch of do., with leaves and cones.
  2. Seeds (natural size).
  3. Do. (magnified).
  4. Do. (magnified in profile).

On the Colouring Matter of the Rottlera tinctoria. By THOMAS ANDERSON, M.D., F.R.S.E., Regius Professor of Chemistry in the University of Glasgow.

The colouring matter of the Rottlera tinctoria has long been an article of commerce in India, and is still farmed by Government, being in considerable demand among the Mahommedan population for dyeing silk. No attempts have as yet been made to introduce it into European commerce, an impression appearing to have existed that the supply is too limited to make it of importance. Dr Cleghorn of Madras, to whose kindness I owe the specimen examined, assures me that this impression is unfounded, and that very considerable quantities might be obtained, if it were likely to prove useful; and the trials I have made with it are sufficient to show that it really merits the attention of silk-dyers. Of its chemical composition very little is known, the only person who has as yet examined it being Solly, and even he appears to have done no more than substantiate the fact that the colouring matter is extracted by alcohol, and has the character of a resin.

The Rottlera tinctoria is a large tree, which is stated by Roxburgh* to be confined to the mountainous districts of the Northern Circars. Subsequent researches, however, have shown that it is very widely distributed over the whole Indian Peninsula, from Ceylon to the north-west provinces; and Dr Cleghorn has found it very abundantly in the hill jungles of Mysore, Canara, and Malabar, from whence large supplies might easily be obtained. The bazaar price of the colouring matter, during the years 1847-8, ranged from 5 to 7 rupees per maund of 25 lbs.

The fruit of the Rottlera tinctoria is about the size of a pea, and is covered externally with curious stellate hairs and coloured glands, which are easily rubbed off, and then form a red powder, which, without further preparation, forms the article of commerce. Similar red glands are found, though in small numbers, on the leaves. The root is also said to be used in Bengal as a dye, but Dr Cleghorn has never heard of its employment in Mysore. The colouring matter, as it

---

* Coromandel Plants, vol. ii., p. 160; and Flora Indica, vol. iii., p. 827.

came into my hands, was a perfectly uniform, dark brick-dust coloured granular powder, perfectly dry, and resembling a very fine red sand. It has a slight taste, and a peculiar, though faint, aromatic odour. It repels water, and is not easily moistened by it, requiring to be shaken with it for some time, and is so sparingly soluble, that even after boiling with it, water acquires only a pale yellowish tint; but the addition of an alkaline carbonate, or, still better, of a caustic alkali, causes the fluid to assume a fine red colour. When boiled with alcohol, the greater part of it dissolves, forming a dark-red solution, and if this be filtered hot from the insoluble matter, it deposits, on cooling, a pale flocky matter, which is sometimes so abundant that the fluid is completely filled by it, while a dark red resin remains in solution, and may be obtained by evaporation as an amorphous mass. The case is different if ether be employed as the solvent; a fine red solution is then obtained, which gives no flocks on cooling, but on sufficient concentration, and standing for a couple of days, solidifies into a mass of granular crystals, to which I give the name of Rottlerine.

By repeated digestion with hot alcohol, the whole of the colouring matters are dissolved, and a pale whitish matter, consisting of cellulose and albuminous compounds, is left. A proximate analysis, in which the proportion of albuminous matters was ascertained by a determination of nitrogen, showed the composition of the powder to be

Water,	3·49
Resinous colouring matters	78·19
Albuminous matters,	7·34
Cellulose, &c.,	7·14
Ash,	3·84
	100·00

In addition to these substances it contains also a small quantity of a volatile oil, and apparently, also, of a volatile colouring matter; for the alcohol which has been boiled with it, after being separated by distillation, retains the smell of the Rottlera, and has a decided yellow colour.

Rottlerine.—If the colouring matter be boiled with ether, and the filtered fluid evaporated to dryness, it leaves only an amorphous mass: but if it be distilled to a small bulk, and left for a day or two in a cool place, as already mentioned, it becomes filled with granular crystals. These are collected on a filter, and pressed between folds of filtering paper, to remove the resinous mother liquor. The pressed mass is re-dissolved in boiling ether, and the crystals deposited, on cooling, are again expressed, and this is repeated until it is obtained entirely free from resinous matters. It then forms a mass of yellow crystals, having a fine satiny lustre, which is seen to great advantage when a fluid containing it in suspension is stirred. Under the microscope these crystals are seen to be minute plates, which are much broken up, and show no well-marked form. Rottlerine is insoluble in water, sparingly soluble in cold alcohol, more so in boiling. In ether it is readily soluble. It dissolves in alkaline solutions, with a dark-red colour. Its alcoholic solution is not precipitated by acetate of lead. Bromine instantly decolorizes it, with formation of a substitution product, which dissolves readily in spirit, and is thrown down by the addition of water. It does not crystallize, and could not be obtained in a state of purity. Nitric acid oxidizes Rottlerine, forming at first a yellow resinous matter, and by longer continued action, a quantity of oxalic acid. Concentrated sulphuric acid, in the cold, dissolves it with a yellow colour, which, on the application of a gentle heat, first becomes red, and finally very dark, sulphurous acid being evolved. Heated on the platinum knife, it fuses into a yellow fluid, which decomposes at a higher temperature, giving off pungent fumes, and leaving a bulky charcoal.

The specimens employed for analysis were from different preparations, and the results were:—

I.	4·332	grains of Rottlerine gave
	11·026	... carbonic acid, and
	2·235	... water.
II.	4·295	grains of Rottlerine gave
	10·885	... carbonic acid, and
	2·175	... water.
III.	4·150	grains of Rottlerine gave
	10·505	... carbonic acid, and
	2·096	... water.

IV. { 4·460 grains of Rottlerine gave
11·275 ... carbonic acid, and
2·200 ... water.

	I.	II.	III.	IV.
Carbon, . .	69·37	69·11	69·03	68·94
Hydrogen, .	5·73	5·62	5·59	5·26
Oxygen, . .	24·90	25·27	25·38	25·80
	100·00	100·00	100·00	100·00

The formula $C_{22} H_{10} O_6$, is the nearest expression of these results, as is seen by the following comparison of the mean of experiments with the calculation,—

	Mean.	Calculation.		
Carbon, . . .	69·112	69·47	C_{22}	132
Hydrogen, . .	5·550	5·26	H_{10}	10
Oxygen, . . .	25·333	25·27	O_6	48
	100·000	100·000		190

The attempts which I have made to confirm this formula, have not led to any definite results, as Rottlerine is incapable of forming compounds with the metallic oxides, and though it gives a substitution compound with bromine, its properties, as already mentioned, were not sufficiently marked to afford any guarantee of its purity, and it could not be obtained of definite composition.

Pale Substance obtained by Alcohol.—It has been already mentioned, that the hot alcoholic solution of the Rottlera deposits on cooling, a pale flocky substance. These flocks were separated from the dark-red mother liquor, and dissolved in the smallest possible quantity of boiling alcohol, from which they were deposited on cooling, in such abundance, that the fluid became nearly solid. By repeated solution, it was obtained of a very pale colour, nearly, but not altogether, white. When collected on a filter, it dries up into masses, resembling the hydrate of alumina, mixed with a small quantity of oxide of iron. It is insoluble in water, readily soluble in hot, sparingly in cold alcohol, and scarcely at all in ether. When examined under the microscope, it is found to consist of minute grains, entirely devoid of crystalline structure. It gives no precipitate with lead or silver salts, and does not appear to from compounds with any other substances, but the quantity at my disposal was too small to permit any detailed examination; for the flocks, notwithstanding their bulk, shrink int nothing when dry. Its analysis gave the following results,—

I. 5·390 grains of the pale substance gave
13·995 ... carbonic acid, and
5·118 ... water.

II. 4·470 grains of the pale substance gave
11·595 ... carbonic acid, and
4·217 ... water.

	Experiment.		Calculation.		
	I.	II.			
Carbon, ...	70·81	70·74	71·00	C_{40}	240
Hydrogen, . .	10·50	10·45	10·05	H_{34}	34
Oxygen, ...	18·69	18·81	18·95	O_{8}	64
	100·00	100·00	100·00		338

These results correspond very closely with the formula $C_{40}\,H_{34}\,O_{8}$, but the impossibility of forming compounds prevents its confirmation.

Resinous Colouring Matter.—The red alcoholic solution from which the pale substance has been separated, leaves on evaporation a dark-red amorphous resin, which melts at 212°, and solidifies on cooling. It dissolves in alcohol and ether, in all proportions, but is insoluble in water. It gives with acetate of lead a dark orange red precipitate, which could not be obtained of definite composition, compounds being formed in different operations, and with various modifications of the process, containing from under 19 up to 34 per cent. of oxide of lead; and even when the precipitation was effected in precisely the same manner, the quantity of oxide of lead varied as much as 4 or 5 per cent.; for this reason I did not pursue the investigation of this substance, which most probably contains more than one resinous acid. I give, however, two analyses, which were made at an early period of the investigation, on the resin dissolved in ether, and dried at 212°.

I. 4·298 grains of the resin gave
10·930 ... carbonic acid, and
2·462 ... water.

II. 4·298 grains of the resin gave
10·890 .. carbonic acid, and
2·415 ... water.

	Experiment.		Calculation.		
	I.	II.			
Carbon, . .	71·47	71·22	71·71	C_{60}	360
Hydrogen, .	6·37	6·21	5·97	H_{30}	30
Oxygen, . .	22·16	22·57	22·32	O_{14}	112
	100·00	100·00	100·00		502

I have added to these the calculation of the formula $C_{60} H_{30} O_{14}$, which agrees tolerably well with the analysis; for although it is not entitled to much confidence it agrees with the determination of the lead salt, giving the smallest proportion of lead. The precipitate in question which was obtained by adding an aqueous solution of acetate of lead to a diluted alcoholic solution of the resin, contained 18·67 per cent. of oxide of lead, while the formula $Pb O C_{60} H_{29} O_{13}$ requires 18·27; but on repeating the preparation in the same manner, I could not obtain sufficiently concordant results to entitle me to fix its constitution. Indeed I think it far from improbable that it may be a mixture of several resinous acids.

The colouring matter of the Rottlera belongs to the class of substantive dyes. It does not require a mordant, all that is necessary being to mix it with water, containing a solution from a fourth to a half its weight of carbonate of soda, and to boil it with the stuff. The Hindoos, in addition to carbonate of soda, which they use in the form of native barilla, employ powdered gum, and before adding water, rub the whole of the materials up with a small quantity of sesamum oil. These additions, however, are not necessary for success, as I obtained a very fine colour without them. It is remarkable, however, that this colour is only produced on silk. Calico, whether with or without a mordant, acquires only a pale fawn colour, and entirely devoid of beauty. On silk, the colour is a rich flame or orange tint, of great beauty and extreme stability. The great brilliancy and permanence of the tint which it produces, and the fact that the material supplied by commerce, contains between 70 and 80 per cent. of real colouring matter, ought to induce the silk dyers of this country to turn their attention to it, the more especially as there is no doubt, that if the matter were placed in the hands of an intelligent person, our Indian empire might supply it in abundance.

The late Lieutenant-Colonel John G. Champion, of the 95th *Regiment.*

When, in the long list of valuable lives which have been sacrificed in the Crimea, we occasionally meet with a name which has been familiar to us in bygone days, as distinguished for learning or science, the grief which we feel for the loss of a brave soldier is sadly deepened by the thought that it is not merely a noble and gallant spirit that is gone, but that stores of learning, trains of reasoning and induction, still going on, habits of skilled observation and industry, are all cut off with the able faculties and cultivated mind to which they belonged. This is the character of the loss we have sustained by the death of Lieutenant-Colonel Champion ; and in recording it in this Journal, these are the qualities which naturally most force themselves upon our attention. Still, when every heart in Britain is beating with pride at the achievements of our gallant soldiers, and when, alas, there is scarcely a hearth whose pride is not chastened by the loss of some dear friend or relative, we cannot confine our notice of him to his scientific labours, but must also give a brief record of his military career, which affords another instance of the truth of the remark often made, that the possession of elegant accomplishments and scientific acquirements, instead of making a worse soldier, is only the more sure indication of military ability.

Lieutenant-Colonel Champion, eldest son of the late Major J. C. Champion, 21st Royal N. B. Fusiliers, was born at Edinburgh, in May 1815. He was descended from a branch of the ancient family of Champion. In 1841 he married Frances Mary Carnegie, eldest daughter of the late Captain David Carnegie, of the 44th regiment. She survives him, with an infant boy and a daughter. He gained his commission at Sandhurst in 1831, and was appointed to the 95th regiment, with which he served uninterruptedly in various climes till his death on the 30th November last. His regiment was at home when the war with Russia broke out, and went with the advance to Gallipoli, thence to Varna and the Crimea. Colonel Champion embarked as Senior Major, and joined General Pennefather's Brigade in the Second or Sir De Lacy Evans' Division of the army. At the Alma, when Lieutenant-Colonel Webber Smith was wounded, the command of the 95th devolved on Major Champion, and he received the thanks of Lord Raglan for his conduct, in a despatch to the Duke of Newcastle, dated 31st October. Major Champion conducted the command of the 95th during all the subsequent harassing operations. On the 26th of October, when the Russians made an attack on the Second Division, they were met by a prolonged resistance from the pickets commanded by Majors Champion and Eman,—so skilfully conducted as to elicit the warmest praise from his General, Sir De Lacy Evans, in his despatch published by Lord Raglan,—and this gallant defence was considered by his comrades of the army to have been a service in which his ability as an officer was eminently displayed. On this occasion he kept the enemy at bay by a close musketry fire, until the ammunition of his men was expended. Afterwards, at one time, he repulsed them by charging; at another, he stopped them by making his men cheer loudly as if reinforcements had arrived, and by such devices he maintained his ground till the welcome sound of the guns crowning the hill at last relieved him from his perilous position. The gallant way in which he held these pickets enabled him to keep the Russians at bay till the division came up in order, and drove the Russians back, who were driven to the walls of Sebastopol by the 95th regiment.

On the morning of the battle of Inkermann, Major Champion entered the field in support of the 41st regiment with a wing of the 95th—they soon met and repulsed the enemy—they were then desired to hurry on to the assistance of the Grenadier Guards, at a battery where the enemy pressed them hard. Conjointly, these brave men (Guards, 41st, and 95th) successfully resisted the persevering attacks of the overwhelming numbers of the enemy. It was towards the end of this struggle, when their ammunition was all expended, that Major Champion (then, we believe, senior survivor) proposed to some of the band of heroes to mount and charge over the battery. This they did in style, and drove the enemy down the hill after a long and most deadly struggle hand to hand. It was at this moment of victory that Major Champion received his death-wound from a musket ball through the breast and lungs. He was taken from the field, and reached the hospital of Scutari. His brilliant conduct in this his last action received honourable acknowledgment from Lord Raglan in his despatches, and in the *Gazette* of 12th December, where he was raised to the rank of Lieutenant-Colonel for distinguished services in the field, but the acknowledgment and the honour came too late to reach the ear of him who had so nobly gained them. Death had put a period to his sufferings on the 30th of November.

Colonel Champion's taste for Natural History developed itself very early; when yet a boy he devoted himself to Entomology, and with Kirby's *Monographia Apium Angliae* as his guide (one of the first scientific works which fell into his hands) he made a very complete collection of Scottish bees. Shortly after he joined the army his regiment was ordered to Corfu, and he eagerly availed himself of the wider field there offered to him, and made and brought home to this country a large collection of insects of all orders from the Ionian Isles. His attention next was turned towards Botany, the branch of Natural Science to which he ever after remained most devoted, and the next foreign station to which he was ordered being Ceylon, he had there ample opportunity of indulging that predilection. He there became acquainted with the late Dr Gardner, then superintendent of the Botanic Garden at Peradenia, and by him was confirmed in his nascent taste for Botany. Whilst in Ceylon he collected much, explored many unexamined parts of the island, and discovered several very curious novelties which he carefully studied and made drawings of in the living state. Some of these he published in conjunction with Dr Gardner, and others have been since described from his materials. He also prosecuted his researches in Entomology with equal vigour, and a very large collection of insects (principally coleoptera) was sent by him to this country, the major part of which were presented to the British Museum. The then local Government of Ceylon saw with pleasure men of such ability occupied in investigating the natural productions of the country, and had it in contemplation to encourage and preserve these observations by publishing, at their expense and under their sanction, a Fauna or Physical History of the Island. Captain Champion was to have been intrusted with the Entomology and a share of the Botany, and he amassed a vast amount of materials for this purpose. A large volume of carefully executed figures of insects was prepared by him, and an immense mass of drawings and dissections of plants. Unfortunately, the change of Government put a stop to this enlightened project, and the materials still remain unused. Let us hope that they may yet be made available to science.

The next scene of his labours was Hong Kong, where he was stationed for three years. This was in a great measure virgin territory, and amply repaid his industry and skill. Along with Mr Bowring he thoroughly investigated the entomology of the island, and their researches were rewarded with the discovery of a great many unknown species. Among others, no less than seven new species of Paussi were detected, all of which (we believe) are now in the British Museum. It would be out of place to dwell here on the new species made known by him; but we must not forget one of the most beautiful of them, a lovely longicorn, described by Mr Adam White under the name of Erythrus Championi.

His botanical explorations were not less fortunate; and we have the privilege of quoting a short passage, giving an account of the results of his labours, taken from a letter from the eminent botanist Mr Bentham, who was more associated with Colonel Champion in the botany of Hong Kong than any other person. Mr Bentham says: "The greatest contribution Major Champion has made to the cause of science was during his three years' residence in Hong Kong, where he collected nearly 500 species, exclusive of Glumaceæ and Ferns, —a most extraordinary number for so small an island, considering, especially, the large proportion of entirely new forms it included. He published several of these, in conjunction with Dr Gardner, in the last paper transmitted by the latter eminent botanist from Ceylon previous to his decease, and inserted in the Kew Journal of Botany. On his return to this country, Major Champion most liberally presented to me a complete set of his Hong Kong collections, communicating to me at the same time his notes and sketches made from the living plants. From these materials I have been drawing up a Florula Hong-Kongensis, the greater part of which (the whole of the Dicotyledones) is already published in Sir W. Hooker's Kew Journal of Botany, and I hope very soon to complete the remainder. The original specimens are deposited in my Herbarium, now the property of the Royal Gardens at Kew. Major Champion, before his departure to the East, presented the set of specimens he had reserved for himself to Sir W. Hooker, who had already purchased from Dr Gardner's executors those which Major Champion had sent to Ceylon, so that the whole of his collections are now deposited in the Herbaria at Kew, open for inspection and study to all botanists. From Major Champion's seeds some of the handsomest of his Hong Kong novelties, such as Rhodoleia Championi, Rhododendron Championæ, &c., have been raised and secured to our gardens by nurserymen and others to whom he had presented them."

Colonel Champion's disposition was essentially unselfish and liberal, and he parted with everything he acquired only too readily. His first object always was to place the unique and more valuable part of his collections in the possession of the public institutions of his country, where they would be most accessible; after that, whoever took an interest in the subject was supplied with specimens, so long as they lasted. In consequence, the private collections he has left are meagre, compared with what he has bestowed on the British Museum, Kew Gardens, &c. &c.

His readiness to communicate his information was equally great, and his original ideas and remarks were often the means of setting other minds on a scent which led to valuable results. His style of writing was easy and fluent, and well calculated to render every subject he treated of interesting to his readers.

In this sketch of Colonel Champion's scientific character, his private and domestic relations are necessarily omitted; but it may not be altogether irrelevant to mention that he was aided and encouraged in his scientific pursuits by his amiable and accomplished wife, a lady of a congenial spirit, who sympathized with his tastes, and participated in his admiration of the beautiful productions of nature. His little daughter, too, often accompanied him in his botanical rambles in Hong Kong, and it was an unfailing source of satisfaction to him to be able to combine the indulgence of his feelings of love, affection, or friendship, with his attachment to his scientific pursuits.

The late Professor Edward Forbes.

[As every thing connected with the late Editor will be interesting to the readers of this Journal, the present Editors have ventured to insert the following sketch by Dr George Wilson, which has already appeared in the pages of *Blackwood's Magazine.*]

Edward Forbes was born in the Isle of Man in February 1815, and died near Edinburgh on the 18th of November 1854, in his 40th year, six months after his appointment to the Regius Chair of Natural History in the University of that city. His great and varied gifts and accomplishments, his remarkable discoveries, and his singularly lovable, generous, and catholic spirit, made him an object of esteem and affection to a very wide circle of friends, and a still wider circle of acquaintances. All were exulting in the prospect of the long and honourable career which awaited him, when, in the height of his glory and usefulness, he was suddenly stricken by a fatal disease, and died after a brief illness.

The following lines seek to apply, *mutatis mutandis*, to the mystery of the great Naturalist's death, certain canons which he enforced in reference to the existence of living things, both plants and animals. Their purport was, to teach that an individual plant or animal cannot be understood, so far as the full significance of its life and death is concerned, by a study merely of itself; but that it requires to be considered in connection with the variations in form, structure, character, and deportment, exhibited by the contemporary members of its species spread to a greater or less extent over the entire globe, and by the ancestors of itself, and of those contemporary individuals throughout the whole period which has elapsed since the species was created.

He further held, that the many animal and vegetable tribes or races (species) which once flourished, but have now totally perished, did not die because a "germ of death" had from the first been present in each, but suffered extinction in consequence of the great geologic changes which the earth had undergone, such as have changed tropical into arctic climates, land into sea, and sea into land, rendering their existence impossible. Each species, itself an aggregate of mortal individuals, came thus from the hands of God, inherently immortal; and when he saw fit to remove it, it was slain through the intervention of such changes, and replaced by another. The longevity, accordingly, of the existing races can, according to this view, be determined (in so far as it admits of human determination at all) only by a study of the physical alterations which await the globe; and every organism has thus, through its connection with the brethren of its species, a retrospective and prospective history, which must be studied by the naturalist who seeks fully to account even for its present condition and fate.

Those canons were applied by Edward Forbes to the humbler creatures; he was unfailing in urging that the destinies of man are guided by other laws, having reference to his possession individually of an immaterial and immortal spirit.

The following lines, embodying these ideas, contemplate his death, solely as it was a loss to his fellow-workers left behind him: their aim is to whisper patience, not to enforce consolation.

Thou Child of Genius! None who saw
The beauty of thy kindly face,
Or watched those wondrous fingers draw
Unending forms of life and grace,
Or heard thine earnest utterance trace
The links of some majestic law,
But felt that thou by God wert sent
Amongst us for our betterment.

And yet He called thee in thy prime,
Summoned thee in the very hour
When unto us it seemed that Time
Had ripened every manly power:
And thou, who hadst through sun and shower,
On many a shore, in many a clime,
Gathered from ocean, earth, and sky,
Their hidden truths, wert called to die.

We went about in blank dismay,
We murmured at God's sovereign will;
We asked why thou wert taken away,
Whose place no one of us could fill:
Our throbbing hearts would not be still;
Our bitter tears we could not stay:
We asked, but could no answer find;
And strove in vain to be resigned.

When lo! from out the Silent Land,
Our faithless murmurs to rebuke,
In answer to our vain demand
Thy solemn Spirit seemed to look;
And pointing to a shining book,
That opened in thy shadowy hand,
Bade us regard those words, which light
Not of this world, made clear and bright:—

"If, as on earth I learned full well,
Thou canst not tell the reason why
The lowliest moss or smallest shell
Is called to live, or called to die,
Till thou with searching, patient eye,
Through ages more than man can tell,
Hast traced its history back in Time,
And over Space, from clime to clime;

"If all the shells the tempests send,
As I have ever loved to teach;
And all the creeping things that wend
Their way along the sandy beach,
Have pedigrees that backward reach,
Till in forgotten Time they end;
And may as tribes for ages more,
As if immortal strew the shore.

"If all *its* Present, all *its* Past,
And all *its* Future thou canst see,
Must be deciphered, ere at last
Thou, even in part, canst hope to be
Able to solve the mystery
Why one sea-worm to death hath passed;
How must it be, when God doth call,
Him whom He placed above them all?"

Ah, yes! we must in patience wait,
Thou dearly loved, departed friend!
Till we have followed through the gate,
Where Life in Time doth end;
And Present, Past, and Future lend
Their light to solve thy fate;
When all the ages that shall be,
Have flowed into the Timeless Sea.

GEORGE WILSON.

Elm Cottage, Edinburgh,
1st January 1855.

A Description of certain Mechanical Illustrations of the Planetary Motions, accompanied by Theoretical Investigations relating to them, and, in particular, a new Explanation of the Stability of Equilibrium of Saturn's Rings.

By JAMES ELLIOT, Teacher of Mathematics, Edinburgh.*

Orreries, as they are called, have been constructed with much elaborate ingenuity, and rendered capable of exhibiting the motions of the planets to a surprising degree of accuracy; but they are so complicated and cumbrous in their machinery—so constrained in their movements—so totally different from that which they represent, in regard to their moving principles (their toothed wheels, pulleys, and inclined planes being utterly unlike the laws of attraction and inertia)—that they are seldom regarded in any other light than as mechanical curiosities, and are rarely used for *explaining* the subject of astronomy. In them we look in vain for imitations of

> " Heaven's easy, artless, unencumbered plan"

(to borrow a description applied to a higher subject), and long for illustrations more simple, and governed by laws more nearly related to those which govern the planets themselves.

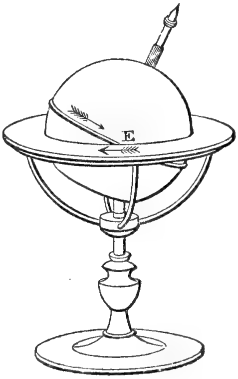
On first commencing the study of astronomy myself, and endeavouring to obtain a distinct conception of that motion of the earth which gives rise to the precession of the equinoxes, it occurred to me that I had seen the same motion in spinning a hoop or a halfpenny. Thence I traced it to the top and the te-totum. Afterwards, in teaching the subject, it appeared to me that, if I could reduce their untractable movements to some degree of management, I might obtain a useful auxiliary to my explanations. There is so much difficulty in imparting to learners a distinct idea of the motion alluded to,—in making them conceive the possibility of a rotation of the earth about its axis in one direction, and

---

* Read before the Royal Scottish Society of Arts, 27th February and 13th March 1854, and the Silver Medal, value Ten Sovereigns, awarded.

a simultaneous revolution of that axis itself, carrying the earth with it in the opposite direction, that we naturally look around for any illustration that can be given of it more satisfactory and more natural than turning the model with the hand. About the same time my attention was also more particularly directed to the same point by meeting with a remark in Sir John Herschel's excellent volume on Astronomy. " A child's peg-top," he says, " or te-totum, exhibits, in the most beautiful manner, the whole phenomenon," of the precession of the equinoxes, " in a manner calculated to give at once a clear conception of it as a fact, and a considerable insight into its cause as a dynamical effect." So far well; but this objection comes in the way—an objection which, of course, the writer just quoted did not overlook—that, in all ordinary tops and te-totums, the motion in question is in the contrary direction to that which we are required to illustrate in the planets, the conical revolution of the axis being, in the former, in the same direction with the rotation, while, in the latter, it is in the opposite direction.

I observed, however, that in tops which have short pegs, this motion—the conical motion of the axis—is slower than in those which have long ones; and, in fact, the shorter the peg, the slower the revolution. It therefore occurred to me that, if we could lower the centre of gravity till it coincided with the centre of motion, this movement would cease altogether, and the top would continue to spin with its axis pointing permanently in any direction in which it might be placed. I also concluded that, if we still further extended the same change which gradually annihilated the positive motion, it would re-appear negative, or in the opposite direction. With that view I had an instrument constructed of the form shewn in the annexed cut, consisting of a wooden ball hollowed out in its lower part, so as to admit the support upon which it rests to be raised above the centre of gravity of the ball, and with a screw



upon its peg, or axis, to admit of its being raised or lowered at pleasure. I also confined it to one place by forming a small cavity on the support for the point of the peg to run in. This being done, I was much pleased to find my expectations exactly realized. By adjustments of the screw the conical revolution could be quickened, retarded, annihilated, or reversed, as might be desired; and all its motions were brought under perfect control. At the same time it was surrounded by a fixed plane to represent the ecliptic, its own equator being marked upon it; and, by forming the axis of hard steel, and giving it a support of agate, its velocity could be kept up without much abatement for a long time.*

The rotation is produced in the ball by means of a string and handle, much in the same way as that in which a humming-top is spun.

The case in which, from the two centres coinciding, the axis remains fixed in one direction without any conical revolution, enables us to illustrate clearly what is meant in astronomy by the *Parallelism of the Earth's Axis*, since the model may be carried by the hand slowly round in any circular or elliptic orbit, without any perceptible deviation of the axis from its original direction.

But, when the centre of gravity is brought slightly below the point of support, we are then enabled to show the deviation from parallelism which arises in the direction of the earth's axis after a long period of years, the same motion exhibiting the *Precession of the Equinoxes.* With the centre of gravity so placed, if the ball is made to rotate in the direction marked by the upper arrow, on the figure, or from west to east, the equinoctial point, E, is observed to move slowly in the direction marked by the lower arrow, from east to west. The latter motion may be made as slow as we please;

---

* Since the model described was constructed, my attention has been directed to Bohnenberger's instrument for the same purpose, of which I was not previously aware. While that instrument is exceedingly beautiful, and adapted to various experiments on rotatory motion, for which the model described above is not intended, it wants (as will readily be admitted) the simplicity and capability of precise adjustment of the latter, and is not so well adapted for the particular purpose of *astronomical* illustration.

so as to approach, within any degree of closeness, the exceedingly slow precessional movement of the earth's equator.

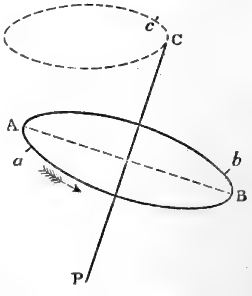
We have thus succeeded in obtaining, in the model, the precise motion of which we were in quest; but if it can also be established that that motion is not only the same as the corresponding motion of the earth, but arises from the same cause, every object will have been attained that can possibly be desired in a model. To establish that, however, will draw us into somewhat abstruse and lengthened theoretical considerations, to which a patient attention must be requested, since they are absolutely indispensable not only to a right appreciation of this particular instrument, but to the elucidation of other parts of the subject.

The first point to be ascertained, then, is—what physical cause produces the conical motion of the axis, either in the instrument before us, or in the common spinning-top? and that question throws us back upon another,—what prevents a spinning-top from falling?—in what way does its motion keep it in an erect position?

A popular notion is that the standing of a top is due to its centrifugal force. The fallacy of that idea is very well exposed by Dr Arnott. He shows that (since the force acts equally on all sides of the axis) if the axis is placed upright, the centrifugal force can have no tendency to incline it to one side more than to another, and can have no more effect in doing so when the axis is inclined. The inclination of the top can have no effect in changing the direction of the centrifugal force, which will still act perpendicularly to the axis, and equally on all sides, neither accelerating nor retarding the fall.

Dr Arnott having shown the fallacy of the opinion that centrifugal force is the cause, substitutes, in its place, another equally fallacious. "While the top," to use his own words, "is perfectly upright, its point, being directly under its centre, supports it steadily, and, although turning so rapidly, has no tendency to move from the place; but, if the top incline at all, the side of the peg, instead of the very point, comes in contact with the floor, and the peg then becomes a little wheel or roller, advancing quickly, and, with its touching edge, describing a curve, as a skater does, until it comes directly under the body of the top, as before." This theory may, at first sight, seem plausible, but is liable to three fatal objections. First,—an inclined cylinder, rolling upon one end, never would roll towards the centre, but, on the contrary, would continually deviate further from it, unless its upper extremity were supported. Second,—the cause would cease, and the top would immediately fall, whenever any small hollow confined its point to one spot, as frequently happens. And, third,—if the standing of the top depended upon the thickness of the point, the finer the point the more difficult it would be to keep up the top; and, if the peg could be ground to a mathematical point, the top would invariably and instantly fall. It is needless to say that such a conclusion is contrary to common observation, which shows us that, in mathematical language, the tendency to fall is no function of the fineness of the point.

In comparing the motions of the top with those of the earth, I thought that I perceived the true reason of the top's standing, viz., that the tendency to fall is converted by the rotation into the conical motion of the axis which I have before described. But, to render this clear, let us commence with the common form of the top in which the centre of gravity is above the centre of motion, and let us suppose, for the sake of simplicity, the top to consist of a single circular plate, or, if we choose, we may take a top of any form, and suppose its whole mass to be concentrated in a single circular section perpendicular to the axis, and the whole weight of that section to be again collected into one circumference, as a hoop around an axis. Further, suppose such a top already inclined to one side, as in the following diagram, CP being the axis, AB the circular section, or rather the circumference, just described, and the arrow pointing out the direction of rotation. The top will then have a tendency to turn over towards that side which is lowest, in doing which, the lowest point, B, of the circumference, would, of course, fall; while the highest point,



A, would necessarily rise. But the point B, in beginning to fall, is, at the same time, carried forward from B to *b*, conveying the tendency to fall with it, so that the actual fall would take place at a point, *b*, immediately in advance of the lowest; at the same time, the highest point, A, beginning to rise, carries that rise forward to a point, *a*, immediately in advance of the highest.* Now let us observe the effect which this has produced upon the top: the point *a*, in advance of the highest, is raised, and the point *b*, in advance of the lowest, is depressed: this change tilts the top over, if I may so express it, aside from its former inclination, bringing the higher extremity of the axis from C to *c*, and making it now lean towards the side immediately in advance of its former position, and, if continued, produces the slow conical revolution of the axis which I have pointed out before, and an accompanying revolution of the lowest and highest point in the circumference, *both* in the same direction as that of the rotation. *Into that conical movement, then, the tendency to fall is converted.*†

---

* No doubt, every point in the semicircumference next B, has a tendency to fall, and every point in the opposite semicircumference A, to rise. But the greatest rise and the greatest fall would take place at A and B, and the united effects of the tendencies of all the points in each semicircumference is the same as if the whole were accumulated at one point.

† This explanation of the standing of a top is not so new as I supposed. When the communication was read to the Society, and subsequently, it was pointed out to me that the same thing might be found in Euler's work entitled "Theoria Motus Corporum Solidorum seu Rigidorum," and also in the works of Poisson and Whewell. I admit it *to a certain extent*, although I was previously ignorant of the coincidence. With regard to Euler, however, his investigation is altogether so obscure that it may be doubted whether the theory of the top can be obtained more easily from the top itself, or from Euler's investigation, supposing it accurate. Throughout the whole of it, I cannot find it distinctly brought out that the top's tendency to fall is converted by the rotation into the precessional (or rather retrocessional) movement. That *seems*, however, to be his meaning, but under symbolical expressions. At the same time he clearly and distinctly assigns a cause of the top's rising to a vertical position, not only different from that which I have given, but different from that which he himself appears to assign as the cause of its not falling. He attributes the rise to friction. In chapter xvii. he says expressly :—" Nunquam enim turbo magis fiet erectus quàm fuerat initio, siquidem nulla affuerit frictio." Now the cause to which I ascribe the rise (whether correctly or not),

But the demonstration is as yet incomplete ; for, although I may have shown that the point which was the lowest at first will no longer be the lowest, unless I can also show that the *new* lowest point will not be lower than that which was previously the lowest point, the top *will* fall, in spite of this secondary preserving motion. How, then, can it be established that it will not be so ? It cannot be proved generally: to do so would be to prove too much; for a top sometimes *does* fall: but the same theory, a little extended, will show *under what circumstances* it will fall, and under what it will not.

The same things being assumed as before, let us further

---

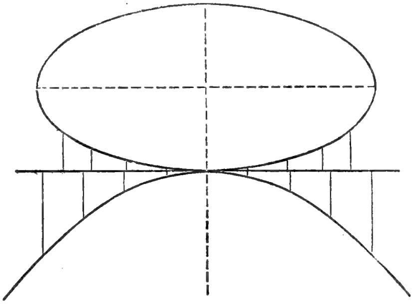
has no connection whatever with friction, and is the very same with that which I have maintained prevents its fall. Practically also I have endeavoured to deprive my model of friction as far as possible, and yet it rises equally well. No doubt the peg is prevented from sliding or rolling from its place by confinement to the agate cup, and if that were what Euler means by friction, or if it served the same purpose, the matter would be simple enough; but he appears himself expressly to say otherwise ; for he goes on :—" At frictio cessare nequit nisi cuspis turbinis in eodem loco persistat," indicating clearly that he does not consider confinement of the peg to a particular place as identical with friction. In fact it is on this last statement that a peculiar position is taken by a writer in the Cambridge Journal (in an article also pointed out to me when the first part of this communication was read to the Society), who attempts to explain and support Euler. He offers to rest the practical proof of Euler's theory on the fact that a top cannot be made to rise when spinning on a very fine point. I showed to the Society a top rising to a vertical position, and spinning perfectly well on the point of a fine sewing needle.

Poisson is much more clear in regard to the conversion of the fall or rise into the conical motion of the axis, but I cannot find that he enters into any explanation of a top of the common form (that is, with the centre of gravity above the point of support) rising towards a vertical position. Still his demonstrations are quite sufficient for establishing my main point, the identity of the top's motions with those of the earth in their principle; and if I had seen his work previously, I might have satisfied myself with quoting it, instead of entering so fully into the subject.

Professor Whewell follows pretty closely in Euler's track, adhering to the same cause assigned by the latter for the rising of the top, viz., friction, but putting it forward with hesitation, and not supporting it by any demonstration. (*Dynamics*, Book iii., Sect. ii.)

I have also been referred to the Lectures and Tracts of Professor Airy. These bring out the theory clearly and explicitly with reference to the earth itself: but in regard to *it*, I have advanced nothing as new: my subject is—not the earth, but the model.

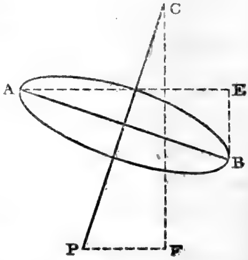
suppose our imaginary circumference to be divided into portions equal to the spaces through which any point moves, in its rotation, in given times. Let us also imagine a vertical plane to touch the circle in its lowest point, and the circle, with the points marked upon it, to be orthographically projected upon that plane, as in the annexed diagram. Again, from the points thus projected upon the circumference of the ellipse, let perpendiculars be drawn to a line touching the ellipse in its lowest point. These perpendiculars will be equal to the abscissæ of the ellipse for the projected points, and set off upon the conjugate axis. Let the same distances be set off upon the tangent line which were previously set off upon the circumference of the circle: these will be the distances through which any point in the circumference would move in the given times, if allowed to advance in a rectilineal direction. Through these points let vertical lines be drawn equal to the spaces through which the lowest point in the circumference would descend in the same times, if the rotation were stopped and the top allowed to fall, turning on its pivot. The curve connecting the lower extremities of these vertical lines will be an approximation to the parabola, and, in fact, for a small portion at the vertex, may be regarded as a parabola, the vertical lines being equal to its abscissæ.



Now, it is a familiar law in dynamics, that, if two forces act upon the same body in the same direction, the resulting force is the sum of the two; but if in opposite directions, the difference; and forces are measured by the motions which they produce in the same mass and in the same time. In the preceding diagram, the perpendiculars on the upper side of the horizontal line show the spaces through which the lowest point in the circumference would be *raised,* in the given times, by the rotatory motion alone; those under the horizontal line show the spaces through which it would *fall* in the same times, if obeying gravity alone. Since these forces, then, are in opposite directions, the resultant force will be equal to their difference, and the resultant motion equal to the difference of the motions which those two forces would produce in the said times. There will, therefore, be a rise or a fall of the lowest point according as the perpendiculars above the horizontal line, or those below, are the greater.

It is, however, the first pair of these perpendiculars—that is, the nearest to the point of contact—which determines the resulting motion: if the first perpendicular, or abscissa, of the parabolic curve be greater than the corresponding abscissa of the ellipse, the lowest point will descend still lower, and the top will fall; but, if less, the lowest point will attain a higher place, and the top will rise towards an upright position.* Now, since the form of the ellipse, corresponding to a given inclination of the top, is constant, while that of the parabola widens or contracts as we increase or diminish the velocity, it is evident that such a velocity may be given to the top that any abscissa of the parabolic curve shall become less than the corresponding abscissa of the ellipse, and

---

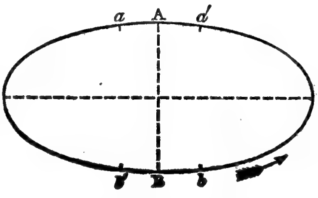


* The tendency to rise or to fall (or rather the excess of tendency in favour of a rise or of a fall) will never cease (the velocity of rotation being constant), but will *continue* to urge the top either to rise towards a vertical position, or to fall to the ground. For, A B being the same circumference which we have supposed throughout, and C P the axis of the top, let the angle of inclination of the top vary: the abscissæ of the parabolic curve above described vary as the force downward (or tendency to fall), and this varies as the sine of the angle of inclination, P C F. The abscissæ of the ellipse vary as the conjugate axis,—that is, as E B; and E B varies as the sine of the angle E A B, or P C F. Therefore the abscissæ of the parabola vary as those of the ellipse. Consequently, if the advantage is in favour of either in any one position of the top, it will continue so in every other; and if the top *begin* either to rise or to fall, it will *continue* to do so, so long as the velocity remains unchanged. But though the *ratio* of the said abscissæ continues the same, their *difference*, when in favour of the ellipse (which difference measures the preponderance of the upward force or of the tendency to rise), will continually diminish. This difference will vary as the sine of the angular distance remaining to be passed over, and ultimately as the distance itself; therefore, I presume the axis will approach the vertical line in an endless spiral, and will never attain an actually vertical position.

that, when such is the case, the top will rise towards a vertical position.

For the sake of simplicity, I have spoken of the distances set off, and consequently the abscissæ and ordinates as of definite lengths; but, to those who have made mathematical subjects their study, it will be evident that, in order to be strictly correct, the second point in each curve must be taken *in immediate succession* to the first, making the first ordinate and abscissa infinitely small. In this case their relative magnitudes may be calculated by means of the differential calculus; or the result may, I think, be shown to depend upon the following principle, which, *if what I have already said be admitted,** will be a self-evident consequence of it. *When the radius of curvature of the ellipse, at its lowest point, is greater than that of the parabolic curve at its vertex, the top will fall; when less, it will rise.*

The same theory applied to that form of the top in which the centre of gravity is below the centre of motion will show that the conical revolution of the axis must then be *backward*, or in a contrary direction to that of the rotation; for in this case the tendency of the top, when at rest, is not to fall but to attain a vertical position. The lowest point, B, in the circumference, having a tendency to rise, and the highest point, A, to fall, both these tendencies will, by means of the rotation, produce their effect in advance of the highest and lowest points, depressing the point *a* and raising the point *b*. If we stop the motion of the top and produce the same effect with the finger, we shall find that the highest point is thus thrown back to *a′*, and the lowest



---

* There will probably be some hesitation in accepting the preceding part of this investigation as strict demonstration. I have the same hesitation myself, and rest nothing upon it. I rather throw it out as a suggestion for consideration. It has at least simplicity in its favour, which Euler's theory assuredly has not. My doubts, however, do not extend to the main point,—of the tendency of the top to fall or rise being converted by the rotation into the forward or backward precessional movement. That does not admit of doubt, and is the only part of the theory which I use for astronomical application.

point also back to b'; and this process, being continued, will produce the *retrograde* conical motion exactly as experiment shows it. In this case we are not required to prove that the top will not fall, since it will not do so when at rest. The only effect of the production of the conical movement will be to retard the tendency towards a vertical position.

When the centre of gravity coincides with the centre of motion, there will be no tendency either to fall or to rise; consequently, no conical revolution; and the top will continue to revolve in any position in which it may be placed, without any change either in the direction or in the inclination of the axis.*

The theory I have thus attempted to establish is borne out, in its main points at least, by experiment, as we have seen in the different movements of the revolving sphere already described. An additional instance is, that our theory leads obviously to the conclusion that, with any given position of the centre of gravity, the more rapid the rotation the slower will be the conical revolution, and that this is at once confirmed by trial with the same apparatus.

The conical revolution of the axis, in the model, not only illustrates that of the earth, but appears to me to depend on the same or a similar cause. There are, however, some objections to this idea, *in limine*, which it may be as well to dispose of first. I have already stated that this motion of the top depends on the relative positions of the two centres. Where, then, it will be asked, are those centres in the case of the earth itself? Before I can answer this I must come into collision with one of our most common, and, I must admit, most useful ideas in physics,—that of *a centre of gravity*. It is one of those hypotheses or theories which we meet with every day, answering very well all ordinary purposes, and

---

* There is another movement of the top, to which in this place I can only briefly allude. It may be called its *erratic motion*. When the pivot is not confined to a point, but running upon a smooth and level surface, with the axis inclined, the top describes a circular orbit, by no means capriciously, but subject to given laws. Its periodic time is the same as that of one revolution of the equinoxes, and its diameter is a fourth proportional to the time of one rotation on the axis, the time of one revolution of the equinoxes, and the diameter of the point of the peg where it rolls on the table.

yet only approximately true. Such a thing as a fixed centre of gravity exists not in nature, or at least but one,—the centre of the whole material universe. The idea of a constant centre of gravity in any particular body depends upon the supposition that the force of attraction is equal at all distances from the attracting centre. Let us conceive a straight uniform rod to be placed in a vertical position, and divided into two equal parts: the lower half will be the heavier, because nearer to the earth's centre; the centre of gravity will therefore be below the middle of the rod. Let us now conceive the position of the rod to be reversed, the upper end exchanging places with the lower: the centre of gravity will then be on the other side of the middle—in that half which was formerly the higher—it will have changed its position in the rod. It follows, then, that in any mass of matter the centre of gravity will not be a fixed point, but will depend upon the position of the mass, and that it will always be in that side of the mass which is nearest to the attracting body.

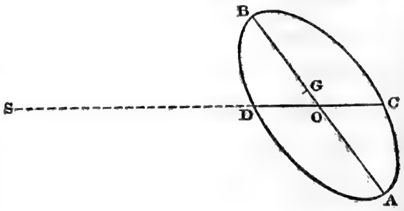
The *centre of momentum*, however, though commonly confounded with the centre of gravity, is not the same; but as this is a rather nice point, and not usually taken notice of, I hope to be excused in saying a few words in explanation.

If two solids, of the same size, differ in weight from a difference in their specific gravity, the heavier has the greater momentum; but if they are alike both in size and in specific gravity, and their difference in weight is caused by a difference in their height above the surface of the earth, then their momenta are equal notwithstanding their difference in weight. Thus, if a cannon-ball were fired from the summit of a mountain, it would strike an object with as much force as it would do if fired, with the same velocity, at the level of the sea, although the weight would be much less.

Apply this now to the case of the straight rod. The lower end is the heavier, but it has not the greater momentum. The centre of momentum is therefore in the middle of the rod, while the proper centre of gravity is not so. In fact, what we commonly call the centre of gravity is truly the centre of momentum. The same reasoning applies to the earth in reference to the sun's attraction: its centre of momentum—the centre round which it revolves in its diurnal motion—is the centre of the sphere (or spheroid); while the varying centre of gravity is always within the hemisphere nearest the sun. Here, then, is the very desideratum supplied, to complete our analogy between the earth's motions and those of the top: here are our two centres,—the one the centre of the mass, the centre of momentum,—the other the proper centre of gravity.

The next difficulty is this. Sir Isaac Newton, as is well known, has demonstrated that the conical revolution of the axis would not belong to the earth were it a perfect sphere, but that it is indebted for it to its spheroidal form; whereas the same motion in the top is independent of its form. The reply to that is, that there is no tendency to such a motion in the top while in a vertical position,—that is, when its centre of gravity is directly above or directly below its centre of motion, because then there is no tendency either to fall or to rise, and that the same thing precisely would be the case with the earth if it were a perfect sphere: the centre of gravity would then be directly between the sun and the centre of momentum. But, in the case of a spheroid, the centre of gravity will be a little out of that line, producing a tendency to fall into it, and this tendency is converted into the motion in question.

Thus, let the point O be the centre of the spheroid AB, and consequently its centre of momentum: let the line AB be the transverse axis, and S the attracting body; and let the spheroid be divided into two half spheroids by a plane, CD, coincident with the line OS, and perpendicular to the plane BOS. Then, since the half spheroid, CBD, is nearer to S than the other half, CAD, the centre of gravity, G, will be in the former half, and consequently out of the line OS.



Therefore the axis, AB, will be drawn in towards the line CS, while no such tendency would arise in the sphere, and consequently no conical revolution of the axis.

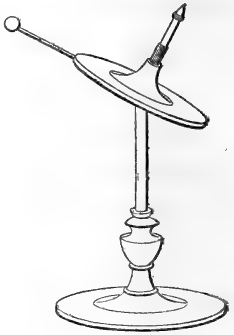
These two objections being set aside, it will readily be perceived that the tendency of the line AB to fall into the line OS, exactly corresponds to the tendency of the top to fall, or to the tendency of the revolving ball of the model, when its centre of gravity is below the point of the pivot, to bring its axis into a vertical position, and, consequently, that the same results must follow. The conical motion produced in the axis of the ball corresponds to that which goes on in the axis of the earth, not only in its existence but also in its cause.

Thus, then, we have two motions of the model agreeing with two corresponding motions of the earth,—viz., the *Rotation* and the *Precession of the Equinoxes*, which is identical with the conical motion of the axis. If we place the instrument, while in motion, upon a stand, and suspend the stand by a cord, from a great height, we may then, as is well known, exhibit the *Elliptical Orbit*, and also the *Progression of the Apsides.*

If we next load the sphere, on one side, *very slightly*, by any means, we obtain an illustration of the *Nutation of the Earth's Axis*, the axis making a multitude of minute conical revolutions round the circumference of the greater conical revolution. Neither are the causes very different in the model and in that which it represents; for the moon's attraction does to the earth what the little weight does to the model: it loads it on one side. The periods of the motions, however, are different in the two; for, in the earth, the period is a lunar month; in the model, a day. This cannot easily be avoided; although, if desired, the period *might* be obtained strictly correct by means of a revolving magnet, representing the moon, acting upon the iron circle which forms the equator of the model earth.

The next motion illustrated by the apparatus is the gradual *Diminution of the Obliquity of the Equator to the Ecliptic.* In the case of the earth itself, Laplace has computed that the diminution is not permanent, but confined within certain limits both of time and of extent, the obliquity, after a long cycle of years, again increasing. In the case of the model, however, the obliquity continues to diminish, an upright position of the axis being constantly approached, but, as I have attempted to demonstrate in a previous note, never attained. The reason of the difference it is not easy to perceive.

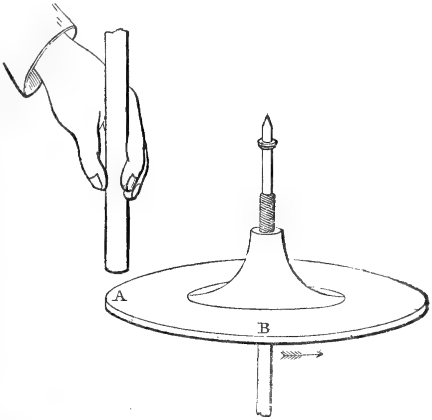
The next piece of apparatus is intended to exhibit the *Retrogradation of the Moon's Nodes.* That phenomenon is similar to the precession of the equinoxes, both in its description and in its cause. In the model, we have the same similarity: we have the plane of the moon's orbit occupying the same place which the earth's equator occupied in the first experiment. Like the plane of the equator, it is inclined to the plane of the ecliptic, the two points in which it cuts that plane, called its nodes, corresponding exactly to the equinoxes in the other case. When the line of the nodes is in the same line with the centres of the earth and sun, we have eclipses of the sun and moon; when otherwise, the moon passes its change and full without eclipses of either luminary. The line of the moon's nodes revolves in a directio contrary to that of the moon's revolution round the earth. There is the same difficulty of conveying a clear idea of this motion by mere words to students of astronomy, that there is in the precession of the equinoxes; but all the difficulty disappears when we can actually show the movement, and that not under the constraint of wheels and bars, but under the impulse of gravitation and inertia.



The cause of the peculiar motion in the model is *similar* to that which produces the corresponding changes of the direction of the plane of the moon's orbit itself, but is not precisely identical with it, inasmuch as, in the model, the force of attraction acts upon the whole plane, while, in the reality, it acts only upon the moon whilst moving in that plane. But the difference is more in appearance than in kind, since we may conceive the solid orbit in the model to consist of a multitude of moons; and, since the effect upon each must be the same, the result will be the same, as in the case of a single moon. The effect will not even be magnified, since the inertia is increased in the same proportion in which the attraction is increased.

Another application of the same model is, to exhibit and illustrate the various kinds of *Perturbation* which one planet exercises on another's orbit. As in illustrating the moon's motion, we use, in this case also, instead of the planet itself, the plane of its orbit, or, more correctly speaking, a solid disc of iron in the position of that plane. The place of the disturbing planet is supplied by a magnet.

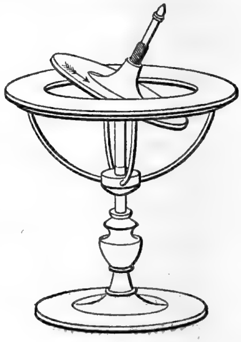
Before entering on the astronomical application of the experiment, it may be curious to observe the peculiar way in which the disc is affected by the magnet. When the disc is at rest, if we bring the magnet near it, either above or below, it is immediately attracted by the magnet and brought into collision with it. But if the disc is made to rotate with sufficient rapidity, and the magnet is again brought near it, as before, it now *seems* no longer to be attracted by the magnet, but rather appears to evade it: you might almost be persuaded that the magnet had a repulsive effect upon the disc.* But it evades it in different ways (at least apparently), ac-



---

* The peculiar fact of the rotating iron disc evading the direct action of the magnet, was first shown to me by a friend, but with no perception, on his part, either of its cause, or of its connexion with astronomy.

cording as we present the magnet to it in different positions. When the disc is revolving horizontally, the magnet, presented above or below any point of the circumference, converts that point into one of the nodes, the half orbit in advance of that point inclining upward or downward towards the magnet, and the other half receding from it. Thus, in the preceding diagram, the direction of rotation being from west to east, as indicated by the arrow, and the magnet being suspended over any point A, that side of the disc does not rise towards the magnet, as we might expect, but the side B does so, a quadrant in advance, while the semicircle opposite B sinks. Again, when the orbit is already inclined, if we place the magnet above the highest point of the disc, or beneath the lowest, the effect is not to increase the obliquity of the orbit, as we should anticipate, but to produce a progressive or forward motion of the nodes. If we present it below the highest point, or above the lowest, it does not diminish the obliquity of the orbit, but causes a retrogradation of the nodes. If we apply it below the descending node, or above the ascending, we do not draw that side towards the magnet, as would appear likely beforehand, but we increase the obliquity of the plane to the horizon: if above the descending node or below the ascending, we diminish the obliquity.* When a circular plane, to represent the ecliptic, is fixed round the revolving disc, as shown in the annexed woodcut, the results are more quickly made manifest, and a screw upon the axis of the disc enables it to be adjusted beforehand, so as to be free from any forward or backward motion of the nodes independent of that caused by the influence of the magnet.



Now, the various effects just described are precisely those

---

* A slight touch of the finger on the revolving disc produces exactly the same effect as the magnet applied on the opposite side. The finger must be kept upon the disc with a gentle pressure, rubbing smoothly over it, so as not to stop it.

which the attraction of one planet produces upon the plane of another's orbit, in the eight different positions described.

The *cause*, in the case of the metallic disc, is exactly the same as that which, we saw, produced the precession of the equinoxes. The point immediately under the magnet is attracted by the magnet, but the effect, in consequence of the rotation, takes place in advance of that point. The very same cause produces the reality represented by the model, in the case of the planets. The action of the magnet upon a metallic plate is not really different from the action of one planet upon another, as far as the plane of the orbit is concerned; for we may suppose, as we did before in the case of the moon, every part of the plate to be a planet; and the magnet influences each part, as it passes it, in the same manner that the one planet influences the other.

The circumstance of the magnet's being applied nearer, and more perpendicular to the orbit, than in the case of the planet, does not affect the result except in degree. It is brought near in order to make the effect more apparent; and, as to the perpendicular direction of its action, it may be remarked that, in the case of the planets themselves, the attraction of the disturbing planet, when not in the plane of the other's orbit, may be resolved into two forces—one in the direction of that plane, and the other at right angles to it: the former is employed in changing the form of the orbit, the latter in changing its direction: it is the latter that is represented by the magnet; and, since it is actually perpendicular to the plane of the orbit, the position of the magnet truly represents it.

The effect of the force in the direction of the plane of the orbit, in altering the form of the orbit, might also, perhaps, be shown by means of a magnet acting upon a loose chain, previously made to revolve as a circular ring by centrifugal force; but the result I have not found to be sufficiently decided to be easily observable by the eye.

I come now to my final application of the same principle which has pervaded all the previously described illustrations of the planetary motions. It is well known to those who have studied the subject, that the theory of Saturn's ring involves a difficulty from which it has never yet been satisfactorily freed. It is agreed on all hands that the assumption and maintenance of the annular form is due to the centrifugal force arising from its rotation; but the difficulty is of another kind; it has arisen from a supposed demonstration by Laplace, in which, as far as I am aware, all other astronomers have acquiesced—that a uniform ring, revolving round a centre of attraction, will be in equilibrium only when the attracting centre coincides mathematically with the centre of the ring,—that, consequently, the equilibrium is unstable; so that, if either the attracting object or the ring be displaced in the least, they will inevitably approach each other till they come into collision. But, though the planet Saturn were poised, with mathematical accuracy, in the centre of his ring (a circumstance without a parallel in astronomy), the nice adjustment would not continue a single day, for it would be immediately disturbed by the varying influence of the other planets and of its own satellites. And not only are there abundant and constant causes to disturb that adjustment, if it existed, but it has been shown, as Sir John Herschel states, " by recent micrometrical measurements of extreme delicacy, that no such adjustment exists, but that the centre of the rings oscillates round that of the body, describing a very minute orbit."

If, then, according to Laplace, there is no stability in the equilibrium of a uniform ring, it follows that, unless there were some preserving contrivance—some counteracting circumstance, that beautiful mechanism would inevitably fall to pieces. For this purpose, Laplace has recourse to the expedient of supposing that the ring must be loaded on one side. That load, having of itself a tendency to describe an elliptic orbit round the planet, like a satellite, will drag the rest of the ring with it. The motion will thus belong to the load, the ring, large as it is, being merely its encumbrance.

To that hypothesis, there are serious objections. In the first place, the load is almost hypothetical; for although some slight apparent inequality may be observed in the different parts of the ring, yet nothing to justify Laplace's idea,—nothing which could be regarded as sufficient to bring about the result on which he calculates. In the second place, the expedient seems to be destitute of that elegant simplicity so conspicuous in the laws which govern the other parts of the planetary system. In the third place, if that load were carried round, like a satellite, and the rest of the ring dragged round by the load, the period of revolution would not be identical with that of a satellite at the same distance as the ring; for the attractive force exerted upon the load, being equal to that upon such a satellite, and the inertia greater in consequence of the superadded mass of the rest of the ring, the time would be proportionally greater. But Laplace has himself proved that such a velocity of rotation is absolutely necessary to preserve the very form of the ring. Laplace's reply to this objection would probably have been, that the planet's attraction acts upon the ring also, as well as upon the load. But he does not say so; and if he had said so, it would have entangled him in such complicated laws, involving both ring and load, that he could no more have established the stability of equilibrium with these than with the simple uniform ring—in fact much less easily.

Sir John Herschel, dissatisfied with Laplace's hypothesis of the load, is driven into another, which appears to me to be no less objectionable. He thinks he perceives, he says, " in the rapid periodicity of all the causes of disturbance, a sufficient guarantee for its preservation;" or, in other words, if I understand him right, the displacement which one planet or satellite causes, another planet or satellite (or the same on its return) restores. He afterwards compares this to " the mode in which a practised hand will sustain a long pole in a perpendicular position, resting on the finger, by a constant and almost imperceptible variation in the point of support." His idea would be precisely realized, if, for the balancer's hand were substituted some ingenious piece of machinery, with its motions so nicely arranged beforehand as precisely to adapt itself to every foreseen and previously calculated displacement of the pole; or rather, perhaps, the author would say, with the pole so nicely placed at first, that every little nod would come just in time for the counteracting motion of some one of its wheels. But, it may be replied, there is no other position or arrangement among any of the heavenly bodies in which any extreme precision of original adjustment has ever been detected: instead of that, they have been subjected to laws by means of which every little displacement *produces its own remedy*, and is its own restoring cause. This rule has been proved, I believe, to hold good throughout all the other motions of the solar system, principal and subordinate; and, if this were an exception, it would be the only exception, standing out as a solitary example of its own kind. No doubt, if any *necessary* connexion could be shown to exist between the displacing and the supposed restoring causes, the general law would, in this instance also, hold good; but no attempt is made to point out such a connexion; nor can we even form the least conception in what way it can exist. In addition to this, the force necessary to restore an unstable equilibrium is always so immensely greater than that which has destroyed it, especially if any considerable time has elapsed in the interval, that a singularity and complexity in the restoring powers would be required, such as is altogether inconsistent with the general character of the planetary motions, if, indeed, it could not be demonstrated to be physically impossible.* No machine can be made to sustain a balanced pole. The exertion of intelligence alone can do it.

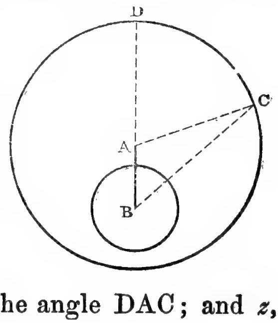
But let us examine Laplace's supposed demonstration of the instability of equilibrium of a uniform ring round a centre of attraction, and see what it amounts to, for if not conclusive, neither his own hypothesis nor that of Sir John Herschel will be necessary. After a very elaborate process of computation, to determine the proportion which the thickness of the ring should bear to its breadth, or at least the limit of that ratio, and a much simpler computation of the period of revolution which each ring ought to have in order to maintain its form by its centrifugal force, showing that that period must be the same as that of a satellite at the same distance, he proceeds to discuss the question of the stability of

---

* I sincerely hope I have not misunderstood the sentiments of Sir John Herschel, an author for whom I entertain the very highest regard. Having examined his expressions carefully and repeatedly, I cannot interpret them in any other sense than that which I have attached to them.

equilibrium, by first imagining the ring to be a mere circular circumference, attracted equally in all parts towards a point not coincident with its centre. He then goes on to compute the attraction existing between the centre of the ring and the centre of the planet, and, by a very refined process of integration, he determines that attraction to be *negative*, or, in other words, that these two centres, instead of attracting, repel each other, and consequently, instead of tending to return to coincidence, will continue to go more apart, until the circumference of the ring touch the surface of the planet.

It is not necessary for me to produce Laplace's calculation, since I am not going to find any fault with it as far as it goes, and its aspect is such that I am confident its attractions, for this assembly, would turn out to be of a negative kind—somewhat repulsive. All that I need to say is easily appreciated; and that is, not that the calculation is wrong, but that one of the principal elements is entirely omitted. *Of the symbols introduced into the calculation, not one has any reference to the rotation of the ring:* it is taken as at rest. How an omission so fatal should have been made on the part of so eminent a mathematician, I cannot explain; neither am I called upon to account for the circumstance of the oversight not having been detected by subsequent astronomers. In the previous part of the demonstration, no doubt, the rotation forms a principal element, but not so in that part which we are now considering. My statement will be found borne out by a reference to Laplace's celebrated work, the Mécanique Céleste, First Part, Book iii., art. 46. But to save the trouble of that reference, I will give a short sketch of his process.



A being the centre of the ring, and B that of the planet; the symbol S representing the mass of Saturn; r, the radius of the ring; ϖ, the angle DAC; and z, the line AB;* he then says,—

---

* The diagram rests on my own responsibility. There is no diagram for this in Laplace's work. It is presumed that no one will seriously maintain that a ring at rest and a ring in rotation, obey the same laws.

$$-\frac{d}{d\,z}\int\frac{\mathrm{S}\,d\,\varpi}{\sqrt{(r^2+2\,r\,z\,\cos\varpi+z^2)}}$$

will be the attraction of Saturn to the ring, decomposed in a direction parallel to z; the integral being taken from $\varpi=0$ to $\varpi=$ the circumference, and the differential being taken with regard to z.

In all this there is no consideration of the ring's rotation, if I can understand it aright. All that is proved is precisely what we should have anticipated without such proof, viz., that if a planet at rest, surrounded by a ring also at rest, were nearer the one side of the ring than the other, it would be drawn towards that side. Still, although this appears *likely* beforehand, it is not self-evident; for, as Sir Isaac Newton has demonstrated, it would not be true in regard to a planet placed within a hollow sphere, which is equally likely. It is very well, therefore, that Laplace *has* demonstrated it, and set the matter at rest, although, from the omission from the calculation, of any element representing velocity of rotation, the result has no bearing whatever upon the actual case of Saturn's ring,* since, as I think I am prepared to show, the very cause of the stability of equilibrium rests on the omitted consideration.

Still, Laplace is not easily understood; and if I have made any mistake regarding his meaning, I shall be glad to be set right.† But, whether or not, it does not affect my result, nor my objection to Laplace's conclusion; for, according to his own explicit statement, the only force he has computed, is that in the direction parallel to AB. I shall at once assume not only that he is right in that in the case of the ring at rest, but also that the rotation of the ring will not affect *that* force. I will therefore commence at the point where he has left off; and start with the assumption that there is a repulsion between the two centres, and, consequently, that, if the

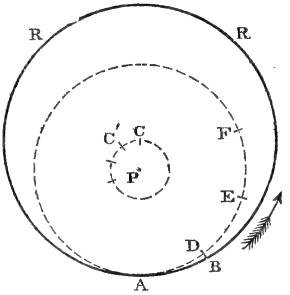
---

* It was, however, especially to *Saturn's* ring that Laplace's investigation was directed, and therefore the rotation was an essential element.

† I have been censured for presuming to dispute so high an authority as that of Laplace. I am not at all disposed to question Laplace's very high position as a mathematical astronomer; but the greatest of men are not infallible, and the subject is certainly a fair one for discussion, so long as I assign my reasons, with perfect willingness to retract if proved to be in error.

ring be displaced in the least, it will have a tendency to approach the planet on that side on which it is nearest to it; but that very tendency will produce its own remedy, by giving rise to another and a very peculiar movement, which I am now about to explain.

In considering the theory of the top, it appeared to me, that, if the top were in the form of a ring, and if an attractive force within it, such as a magnet, were substituted for the downward force of gravitation the ring would avoid falling in towards the centre of attraction by an evasive movement, similar to that by which a top avoids a fall, and similar also to that by which, as we have already seen, the iron disc avoids contact with the magnet above or below it,—that, in fact, the tendency towards the centre would be converted into a slow eccentric revolution of the centre of the ring round the centre of attraction, entirely different from the rotation of the ring itself, exactly in the same manner as the tendency of the top to fall is converted into a slow conical motion of the axis. Thus, in the following diagram, let RR be the ring, C its centre, and P the centre of the attracting power, eccentrically placed with regard to the ring, the nearest point of the ring being A. That point A, is then acted upon by two independent forces,—that of the rotation, carrying it forward from A to B, and that of the attracting power, drawing it from B towards P, and is consequently brought to a point D between B and P, while the centre of the ring passes, in consequence, into the position C′. The same movement continued brings the nearest point successively into the position D, E, F, &c., and carries the centre round the curve CC′.



It is only with a certain velocity of rotation, however, that the curve CC′ will be part of a circle. If the velocity is less than that, the point D will be nearer than A to P, and the ring will become more and more eccentric, till it is brought into collision with the attracting object, corresponding exactly to the case of the top when its velocity is not sufficient to prevent it from falling. But if the velocity is greater than that which is necessary to keep the nearest point of the ring at a uniform distance from P, then PD will be greater than PA, and the ring will become less and less eccentric, the circle, or rather the curve, ADEF gradually enlarging till it coincide with the ring, and the curve CC′ gradually contracting till it disappear in the central point. The ring then becomes perfectly concentric with the planet, and the state of stable equilibrium is restored. The latter case corresponds again to the case of the top whose velocity is sufficient to cause it to rise towards a vertical position.*

Such was my theory, formed independently of experiment, but afterwards confirmed by it. After repeated trials, I succeeded, by means of the apparatus represented in the following drawing, in showing it.

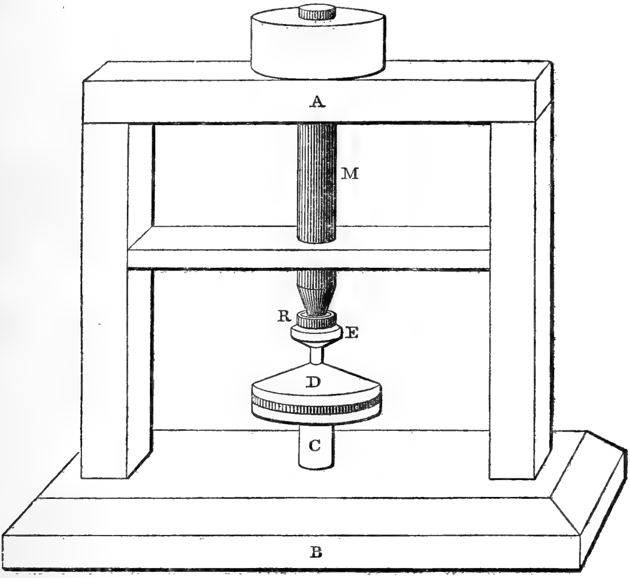
M is a magnet supported on a stand, and R an iron ring capable of revolving rapidly. E is a wooden support to contain the ring. D is an appendage employed for the purpose of bringing the centre of gravity of the whole to the same level with the point of support, and so getting rid of any conical motion which the axis of the ring might have independently of the magnet. The ring R, its support E, and the appendage D, revolve together upon a hollow on the top of the stem C, and are set in motion before the magnet is introduced. It is then found that the ring, when revolving with sufficient rapidity, is not, as Laplace asserts, in instable equilibrium, but that the rotatory motion is able to preserve it from collision with the magnet. We find also precisely the same eccentric revolution which was anticipated by theory, and corresponding exactly to that which, as I have previously stated, *is observed in Saturn's ring itself.*

The power of preserving the equilibrium, in the model, is so decided, that the whole apparatus may be turned considerably on one side, without derangement, the ring accom-

---

* If the top never reach a *perfectly* vertical position, neither will the ring ever become *perfectly* concentric with the planet. But it is sufficient if we establish that it will constantly tend towards that state, approaching indefinitely near to it.

panying the magnet; and, if so turned before the introduction of the magnet, the magnet will *bring* the ring into a concentric position, permitting its introduction into it, *while a non-magnetic bar cannot be so introduced.* The magnet, instead of causing collision, prevents it.*



I have thus, both by theory and by experiment, attempted to explain that phenomenon, hitherto, as I think, not accounted for in any satisfactory manner, and to show that it rests on the same principle as the standing of a spinning-top, the precession of the equinoxes, the retrogradation of the moon's nodes, and the perturbation of the planes of the orbits of the planets. How far I have succeeded I leave others to decide.

---

* The loose structure, or fluidity, of Saturn's ring will not affect my theory, so long as there is sufficient cohesion, or mutual attraction, among its component particles, to keep them together as one body, and sufficient velocity of rotation to preserve the annular form.

REVIEWS AND NOTICES OF BOOKS.

1.—*Die Kreidebildungen Westphalens.* Von Dr FERD. ROEMER. 1854.

2.—*Coupe Géologique des Environs des Bains de Rennes.* Par A. D'ARCHIAC. 1854.

The nature of the change which the cretaceous formation, as a whole, undergoes in its extension from Belgium across Rhenish Prussia into Westphalia and Northern Germany, or southwards across France to the Mediterranean basin, is such, that the English geologist, however well versed in the elementary knowledge of its divisions in his own country, the mineral character and aspect of each, and of the included animal remains, must inevitably find himself at fault when he enlarges his inquiries in these directions. Beds identical in composition, and in the facies of their fossil contents with such as occur in this country at the bottom of the cretaceous series, are just such as in Westphalia may be met with at the top. Mineral character loses its value, and only leads into error, whilst the distribution of fossil forms will be found to be altogether at variance with these laws of sequence and limited duration, which are still reverenced by so many of our palæontological naturalists.

It will not be necessary to borrow much from the descriptive detail of Mr Roemer's work, which recommends itself as much by its fulness as its cheapness to whoever may wish to explore personally a district over which, as he remarks, the cretaceous formation occupies a larger superficial area, and presents a more favourable condition for examination than does any other in Germany.

The divisions and subdivisions of the cretaceous strata of Westphalia, taken in descending order, are as follows :—

A. 1. Upper Sandstone group, consisting of yellow sands, with bands of sandstone, forming the hill group of the Haard, of the Hohe Mark, near Hattern, and of the hills between Klein-Reken and Borken : the gray calcareous sandstone of Dülmen, and the clay marl with siliceous beds of the hills of Cappenberg. Taking the Haard as a type, it presents just such a group of strata as is to be met with in the south-east of England, within the area of the lower greensand, presenting barren hills with tabular summits covered with heath and broom, and composed of alternations of sands and sandstones, with flat and tubular concretions of ironstone ; there are also occasional lines of chert. This upper subdivision is said to be not less than 1000 feet thick. With such a combination of external characters, its place in the cretaceous series would seem to be either with the upper or lower greensand groups; the fossil contents of the beds, however, are both abundant and well preserved, and their relation to the series next beneath is unequivocal. Mr Roemer gives a list of upwards of twenty determined species; with respect to these, we may exclude, as also in all subsequent lists, such forms as seem as yet to be peculiar to the German chalk strata; and taking those only with which we are acquainted in this country, we find

Exogyra laciniata, l. ch., w. ch., Inoceramus Cripsii (rd. ch.) Hunstanton, Cucullæa glabra, (Bldwn), Pholadomya caudata, Belemnitella quadrata (w. ch.), Nautilus elegans (l. ch), Terebratula alata and plicatilis; against which are placed indications of their several positions in the English series.

Mr Roemer considers that this subdivision represents the loose sands, with calcareous bands rich in fossils of the wood of Aix and of the Luisberg, and that it is the equivalent of the uppermost white chalk.

In the lower strata of the hill of Cappenberg are Bourgueticrinus ellipticus (u. ch.); Marsupites ornatus (u. ch.); Belemnitella quadrata, which further support this view.

Next beneath this arenaceous group is

A. 2, A Calcareous Clay series of great thickness, and which, though mainly composed of marly beds, yet presents a mineral change in a given direction; thus, the soft crumbling marls of the east of the basin, as about Beckum, are the equivalents of the compact calcareous beds resembling chalk, which occur at the same level on the west, at about Virden. Like changes occur in lower parts of this cretaceous series; and in the Aix-la-Chapelle district still greater changes take place within still narrower limits.

The strata of the hill of Baumberg, near Münster, have yielded the largest assemblage of fossils, by means of which the hard calcareous strata near Ahaus, the chalk marls north of Coesfeld, and the clay-marls of the hills about and west of Beckum are connected with the same subdivision.

Manon megastoma (l. ch.); Siphonia cervicornis (u. ch.); Cœloptychium agaricoides (u. ch.); Parasmilia centralis (u. ch.); Bourgueticrinus ellipticus (u. ch.); Diadema ornatum, Ananchytes ovata (u. ch.); Micraster cor. anguinum (u. ch.); Chama striata Ignabergensis (u. ch.); Terebratula splicata (u. ch.); Ostrea vesicularis, (u. ch.); Pecten costatus; Spondylus spinosus (u. ch.); Inoceramus Cripsii (l. ch.); T. Lamarckii (u. ch.); Belemnitella mucronata, (u. and m. ch.); B. quadrata (u. ch.); Ammintes Lewesiensis (l. ch.); Turrilites polypocus (l. ch.)

This list might be somewhat extended, by including the species quoted from the numerous localities, described by Mr Roemer, on the north and south of the Lippe; but, taken by itself, it is quite sufficient to indicate a group corresponding with the great mass of our middle and upper chalk, and which is ranged, together with the overlying arenaceous division, under the Senonien group of M. d'Orbigny. The two together form a tract which is isolated from the rest of the cretaceous series by the alluvial and turf formations of the valleys of the Ems and the Lippe, so that their immediate superposition on the next group is nowhere to be seen.

B. 1. The Pläner, whose age and position at one time was such a matter of doubt and difficulty to some English geologists, is, according to M. Roemer, the most constant in its organic remains and mineralogical character of all the cretaceous rocks of New Germany. He estimates its thickness at more than 800 feet; of this, the upper portion is mostly a compact, pure, calcareous rock, the lower a marly calcareous clay. At Bochum, an intercalated band of greensand is seen, and in the range of the Pläner, eastward, such bands increase, and serve to subdivide the formation into two natural groups. In this form it can be traced round the whole of the bay or amphitheatre, from Muhlheim, near the Rhein, to Lichtenau, and thence to Bevergern, near the Ems, and along this whole line it is only separated from the older formations by the intervention of the sands of Essen.

The list of fossil species from this great group is very limited; such as there are occur abundantly. Inoceramus mytoloides (l. is most common; next in frequency is Terebratula pisum; also Tereb. striatula; T. semiglobosa (l. ch.); T. gracilis; T. splicata (m. ch.); Spondylus spinosus (u. ch.); and Holaster subglobosus; Ammonites peramplus (l. ch.); Nautilus elegans. A suite which refers the Pläner to the lower chalk.

B. 2. The Flammen mergel is a name first given to some beds which are seen between Goslar and Seesen, and which occur also in Westphalia. The strata consist of argillaceous limestone, more or less siliceous, of a light gray, with dark streaks. From its hardness, this group usually shows as a projecting edge between the ridges of the Hils sandstone and the Pläner, and is as much as 100 feet thick. It is best seen from Dörenschlucht to Bielefeld. Its geological position, in the Teutoberger Wold, is next beneath the Pläner. The only fossil quoted by Mr Roemer is Avicula gryphæoides (u. gr. s.)

B. 3. *Greensand of Essen.*—Under the head of the "greensand of Essen," Mr Roemer groups a very variable series of accumulations, consisting sometimes of very coarse conglomerates or sandstones, and of fine calcareous marls. They vary much in thickness; their position is constant, being in immediate juxtaposition on the highly inclined beds of the carboniferous groups, and overlaid by the beds of the Pläner, with which the marly beds agree mineralogically.

This remarkable group, commencing at Essen and Mühlhein on the Rhine, and whence were derived so many of the cretaceous forms described by Mr Goldfuss, ranges along the outline of the palæozoic rocks as far as Stadbergen on the Diemel. In this we have a close approximation to an old coast line: in a direction at right angles to this line the detrital beds thin away and become finer; and from west to east the alternations of coarse materials with fine sedimentary matter are more frequently repeated. A like change in the arenaceous band dividing the Pläner, would seem to show that the present bay-like form of the cretaceous map of Westphalia is due to the form assumed by certain lines of disturbed strata, at some pre-cretaceous period. Along the whole of this line the conditions of accumulation are just such as the Tourtia of Belgium presents, of which according to Mr Roemer it is the exact equivalent. If this was ever made matter of doubt owing to the copious marine fauna of the sands of Essen, the difficulty is removed by an examination of the group further east. It is then seen that the assemblage about Essen was due to local conditions, whilst about Bilmerich, with sections which strikingly resemble those of Tournay, the beds contain Arca cardiæformis, and the large Pleurostimaria of that locality.

Mr Roemer give a list of 104 species from Essen. Of these we recognise as British, Scyphia infundibuliformis (Farr.), S. furcata (Farr.), Manon, peziza (Farr.), Tragos pulvinarium (Farr.), Micrabacea coronula (Worm.), Cidaris vesiculosa (u. ch.), Diadema ornatum (u. gr. s. l. ch.), Goniopygus peltatus (u. gr. s.), Ceratomus rostratus (Worm.), Discoidea subuculus (Worm., l. ch.) Catopygus carinatus (Worm. l. ch.), Nucleolites lacunosus (u. gr. s.), N. cordatus (u. gr. s.) Terebratula latissima (Farr. to l. ch.), T. nuciformis (Farr.), T. oblonga (Beaumont),T. nerviensis (Farr.), Ostrea macroptera (O. diluv.), O. carinata (u. gr. s.), Exogyra halotoidea Worm. Blackdn.), E. conica (greensands of the West of England), E. plicatula, Pecten asper (Worm. Blackdn.), P. cretosus (w. ch.), P. laminosus (l. ch.), P. costatus, Spondylus striatus (u. gr. s. Wilts), Nautilus elegans (l. ch.), N. simplex (n. grs. s.), Ammonites varians (u. gr. s.), A. peramplus (l. ch.), A. Mantelli (u. gr. s.), Turrilites costatus (l. ch.)

The inference as to the relative age of the sands of Essen is that, considered according to the British cretaceous series they are the equivalents of the chalk marl, and of the sands which are beneath it; as those of Warminster.

It will be thus seen that round the great cretaceous bay of Westphalia there is a line of littoral sea beds, surmounted by deep-sea accumulations of vast thickness, such as the Pläner marls and limestones; the continuity of which conditions seems to have been more than once disturbed by oscillations such as caused the outspread of the coarse bands which are subordinate to that great group.

To this again succeeds the deep-water sedimentary beds of Mr Roemer's second group, the equivalents of our upper chalk. The chalk formation of the south-east of England is nowhere complete: even where thickest there is abundant evidence of the removal of a much higher portion consisting of flinty chalk; but whether, in its upward extension over the English area, it subsequently underwent any change in mineral character, can only be matter of conjecture. It is most probable that it did not. Over the Westphalian area, on the contrary, the formation of the white chalk was followed by a shallowing; the result of which was that sandy beds, characteristic of moderate depths, were formed above those of the deep-sea chalk; and the zoological result is exhibited in the lists of marine forms given by Mr Roemer, when we have the recurrence of a fauna identical as to many characteristic species with one which had previously existed over the same area at a long distant anterior period, and confirming in a remarkable manner the accuracy of an hypothesis advanced by Mr D. Sharpe (*Geol. Jour.* vol. x. p. 186.)

Gault.—It had been generally supposed that this group was not represented in the cretaceous series of Germany. In a railway cutting near Neuenheerse a section was exposed in which certain strata of a red sandstone overlaid true Neocomian beds, and which from containing an ammonite supposed to be A. auritus has caused the sandstones in question to be referred to the Gault. With respect to the identity of the species, Mr Roemer admits, that it does not altogether agree with the French and English forms. Mr Roemer recognises the Gault at another place from the presence of A. interruptus; but this shell would not be sufficient evidence, and the distinctness of this group cannot as yet be considered to have been satisfactorily made out.

C. *The Neocomian*—is the lowest cretaceous group, constituting a long ridge along the Teutobergervald, but altogether wanting on the south from the Rhine to Winnenberg. It is composed of yellow and red sands, which were formerly supposed to belong to the Quader sandstone; but the discovery of a number of fossil forms, as from Oerlinghausen and Bevergern, has determined its true age and position. On the south, a little beyond Lichtenau, these sandstones are found resting inconformably in the Triassic and Jurassic beds there. Further north they mostly overlie clays containing Cyrena majuscula and Milania strombiformis, well-known forms of the so-called Wealden of North Germany. Its infra-position to the whole of the cretaceous group of the district is also shown by certain protruding ridges; as the hill of Gildehaus near Bentheim, and at Losser just within the Dutch province of Overyssel. The following forms are quoted from the several localities:—Ammonites Decheni-bidichotomus, Belemnites subquadratus, Crioceras Duvallii, Pecten crassitesta, Exogyra sinuata, Thracia Phillipsi, Avicula cornueliana, Perna Mulleti.

In the last Crioceras Duvallii, which is a form peculiar to the Neocomian group of the Mediterranean area, may perhaps be C. plicatilis of the Speeten clay; the rest occur in the Neocomian beds of the S.E. of England and of the N.W. of France. In Hanover Mr Roemer considers that it is represented by the sandstones and conglomerate of Hils.

The transgressive passage of the Essen group, over and beyond the area occupied by the lower or Neocomian group, serves to indicate the direction in which subsidence was taking place in that quarter during the cretaceous period; and confirms an opinion expressed by the late Professor Edward Forbes, after his visit to Aix-la-Chapelle in 1852, that the plant-bearing beds, subordinate to the sands of that locality, were the terrestrial equivalents of the older portions of the cretaceous series.

The second memoir placed at the head of this notice is a description by M. d'Archiac, of a group of strata, of somewhat the same age as those before noticed, and which are well seen near the Baths of Rennes, in the midst of the Cerbières (Department of the Aude). A very large part of the region which separates Westphalia from the south of France was included in that which formed the terrestrial surface of the time of the cretaceous marine series; and the memoir in question is interesting, as it adds much to our knowledge respecting the geographical range and distribution of that Fauna, and suggests speculations as to the source or quarter from which portions may have been originally derived. The Cretaceous Fauna of the European area already presents two such distinct assemblages of forms, that geologists are warranted in distinguishing between the Southern or Mediterranean, and the Northern or Germanic province. It is most probable that the southern of these dates back its origin to times long antecedent to the other, so that the area now represented by the departments of the Var, Hautes, and Basses Alpes, was beneath the waters of the cretaceous ocean during a long lapse of time before the more westerly but adjoining area of France became submerged. The fact of this successive depression is well illustrated by M. d'Archiac's memoir; the cretaceous strata of the Cerbières rest unconformably on old Palæozoic slates, yet in spite of their vast thickness there is no part which can be considered older then the Gault of England, and as such, it can be clearly identified with the beds of that stage which overlies the older (Neocomian) formation in the three departments before named. This agreement, however, is only general: M. d'Archiac remarks, that it is not with the nearest beds of the same age in that part of France that those of Rennes can be compared with reference to their fossil contents, but rather with those of Gosau (Eastern Alps) with the Pläner of North Germany, and the sands of Aix-la-Chapelle; and that, in addition, the list presents identifications with forms from a much more distant region. About ten years ago Professor E. Forbes described a beautiful series of fossils from Southern India. The facies of this Fauna was characteristically cretaceous: of thirty-five species of Cephalopods, three-fourths were found to be very closely allied to species from the Neocomian beds of the department of the Var; many other genera of Mollusks presented a like result. In addition to these species a close and critical comparison satisfied Professor Forbes that there were twelve forms which could not be distinguished from well-known European ones: of these some were limited to the south of France, and some ranged into our own area. M. d'Archiac, whose caution in stating results is well known, has added as many as five or six to this list—making about ten per cent.—a very remarkable result when the difference in latitude, amounting to 30°, is taken into consideration.

The Entomologist's Annual for 1855, *comprising Notices of the new British Insects detected in* 1854. Edited by H. T. Stainton, Author of the "Entomologist's Companion." London: John Van Voorst. 1855.

The idea of an Entomologist's Annual is a good one; and we hail with satisfaction the attempt which has been made by Mr Stainton and his able coadjutors, Mr Smith and Mr Janson. At the same time, we will not receive this from their hands as more than a preliminary essay towards the production of such a work. Our notion of what an Entomologist's Annual ought to be goes far beyond what Mr Stainton sets before him. He tells us that his object has been "to give systematically notices of all the new species found in this country in the past year, and at the same time to intimate which rare species had been taken in any plenty." The latter purpose has not been attempted this year from want of space, but the former has certainly been ably accomplished.

Mr Stainton records no less than 173 species of *Lepidoptera* as detected in Britain since the publication of Stephen's Illustrations of British Entomology in 1835, of which 11 are species hitherto undescribed, and of which Mr Stainton has given good descriptions, and in one or two instances figures. He might have added 15 *Tortricina* to his list, had he taken all those enumerated by Stephens in his Museum Catalogue as British species; but he has most properly abstained from doing so, on the ground that he has no satisfactory information regarding them, reserving to himself to introduce them in subsequent years, should they hereafter prove distinct. Some fine species are recorded in the list, such as *Anthrocera Minos, Spaelotis Vallesiaca, Cerura bicuspis,* &c.

Mr Stainton next gives some observations on the British Tineina, as supplementary to his work on them in the Insecta Britannica, and Entomologist's Companion; and he answers some "enigmas" which he had propounded in the latter work, chiefly relating to the breeding of some of these small moths. A recapitulation of some other of his enigmas, which have not yet been solved, closes his portion of the Annual.

Mr Frederick Smith of the British Museum next takes up the *Hymenoptera.* He adds 59 species to our British bees, as having been noticed since the publication of Kirby's Monographia Apium Angliæ in 1802. With the exception of half-a-dozen, the whole of these seem to have been discovered by Mr Smith himself within the last few years—a testimony to his abilities as an entomologist which our readers will know how to appreciate. In the *Lepidoptera* and *Coleoptera* Mr Stainton and Mr Janson have drawn their materials from numerous sources; but Mr Smith has had the field of Hymenoptera almost entirely to himself. Three of the finest species, however, have not fallen to his lot. *Osmia parietina* fell to Mr Curtis; *Bombus Lapponicus*, Fab. was first found by Mr Newman, and the fine *Bombus Smithianus*, White (*arcticus*, Dahlbon.) was captured in Shetland by Mr Adam White.

Mr Smith adds 6 new species to the fossorial Hymenoptera published by Mr Shuckard, and a like number to the *Myrmicidœ* and *Formicidœ*; and concludes by giving a couple of valuable pages of "notes in explanation of the new species of aculeate Hymenoptera in Stephens' Systematic Catalogue," in every line of which one of Mr Stephens' species is destroyed after this fashion: "*Pompilus nervosus*, Steph., is the female of *P. gibbus*, Lin.;" "*Pompilus basalis*, Steph., is the male of *P. gibbus*, Lin." &c. &c.; and he knocks off 65 of Stephens' species in this way. He does not tell us how he has come to the conclusion he announces, but we presume it is from an examination of Stephens' own type specimens now in the British Museum; and Mr Smith's mere statement of the result to which he has come will, we are sure, be received by entomologists in general as quite sufficient evidence of its accuracy. To such obliteration of Stephens' species and names entomologists are now well accustomed; and they are much indebted to those who undertake the ungrateful task of clearing up the confusion which that celebrated entomologist has unhappily made. Mr Smith has limited himself to the portion of the Hymenoptera above noticed. A separate work on the *Chalcidites* must first be executed before they can contribute their share to new acquisitions.

The *Coleoptera*, which occupy the remainder of the volume, have been undertaken by Mr E. W. Janson, who has bestowed very great care and attention on his part of the work. His list includes all those species which have been noticed as occurring in Britain since the publication of Stephens' Manual in 1839. The sources from which he has drawn his information have been Dawson's Geodephaga Britannica, Murray's Catalogue of Scottish Coleoptera, Hardy and Bold's Catalogue of Northumbrian Coleoptera, Hogan's Catalogue of species found in the neighbourhood of Dublin, and scattered notices published in the scientific periodicals of the day, such as the Annals of Natural History, the Zoologist, &c. He has thus accumulated 227 species, of which he says it is presumed none are given in Stephens' Manual. We fear he is mistaken in saying so. Stephen's descriptions are so vague and undecipherable, especially of the smaller species, and his own collection frequently so inaccurate, that it is often impossible to tell what insect he is describing, and in such cases his names can neither be adopted as principal denominations, nor even as accessory synonymes, and the entomologist is compelled to take the Continental names, so that a number of insects under these names may be included in the list which Stephens had got and thought he had described in his Manual. We wish that Mr Janson had, like Mr Smith, given a list of those of Stephens' species which should be deleted from his Manual, or referred to older well-known names. He would have found much assistance in doing so in the works from which he has compiled his list, especially in Dawson's Geodephaga Britannica and Murray's Catalogue of Scottish Coleoptera, where great attention has been paid to the synonymy of Stephens' species. Perhaps Mr Janson may undertake this in some future Annual. When it shall be done, it will probably be found that instead of adding to the number of species reckoned as British, we must still reduce them.

From the summary which we have given, it will be evident that "the Entomologist's Annual" is as yet very far from fulfilling the promise which its title holds out. It can scarcely be said to do so even as a "*British* Entomologist's Annual," for half the orders of British insects are left untouched. The *Hemiptera, Homoptera, Neuroptera, Orthoptera, Diptera, Aptera*, &c. are never mentioned ; and what has been done in the other orders, although it may fulfil what Mr Stainton in his preface professes to do, certainly fulfils only a very small, and that the most inconsiderable part of what we imagine to be the true duty of an Entomologist's Annual. As we take it, the object of such a work should be not only to bring together the notices of new captures in the course of the year, but to make the entomologists aware of what has been doing in the literature of the science, what new works have appeared, what information is contained in them, and generally to keep the entomologist *au courant du jour* in all matters relating to his study both at home and abroad. British entomologists are in general wofully ignorant of what is going on on the Continent, and to supply this information should be one of the great objects of an Entomologist's Annual. It may perhaps be supposed that such remarks should have been withheld at the commencement of a new work, and that as it goes on in future years the authors will of themselves adopt such a plan as we have indicated, but we learn from Mr Stainton's preface that no such course is contemplated, and we are threatened in another year with a very different kind of supplement. Mr Stainton says, "The object of the present annual is to record systematically the discoveries of each year; but it need not thereby be a purely technical work, and with the view of making it attractive as well as useful, several amusing chapters would have been introduced had space permitted. If the demand should be sufficiently great to warrant such a proceeding, the bulk of next year's Annual will be increased, without any alteration in the price, and I may then be able to give some 'Sayings and Doings at St Osyth,' by Mr Douglas; 'Results of a Summer's Residence at Fochabers,' by Mr Scott; and a chapter 'On the comparative degrees of usefulness of Public and Private Collections,' or other communications of a like nature." We hope Mr Stainton will reconsider this. However excellent the papers alluded to may be in their way, he may rest assured that that is not the sort of thing his readers want. They do not look for amusement in a scientific work, and the best way to make it attractive is to make it useful.

Mr Stainton no doubt foresees that he will have a lack of matter next year; and seeing that the discoveries of the last twenty years have all been required to fill the pages of this small volume, it is undoubted that if he confines himself to the narrow bounds he has laid down, he will not have matter for a dozen pages. But if instead of filling up the deficiency with "amusing chapters," he and his coadjutors will set themselves to give the information we desiderate, he will find that his yearly pages will be all too small to hold the half of the valuable matter he has to communicate. Let them take Erichson's "Report on the Contributions to the Natural History of Insects, &c. during the year 1842," of which a translation was published by the Ray Society in 1845, as a model; and however short they may come of that unequalled production, we shall venture to say that the "Annual" will then give more satisfaction than if it contained the liveliest articles that ever were tendered to a monthly magazine.

Proceedings of the Berwickshire Naturalists' Club. 1854. 8vo. (Printed for Members only.)

Proceedings of the Cotteswold Naturalists' Club. 1853. 8vo.

Malvern Naturalists' Field Club. 1855. 8vo.

The proceedings of the active Club placed first on our list, now in its twenty-third year, besides the annual address of the president, contains valuable information on the Zoology, Botany, and Geology of the district.

The printed papers of this Club extend now nearly to three volumes. Previous to the time of its formation there were man active naturalists in the district, whose circumstances and avocations prevented much interchange of opinions with their fellow-labourers, and it was chiefly at the suggestion and by the energy of Dr Johnston of Berwick, that a plan was formed to increase the communication with each other. "The Club was instituted," the opening paragraph of its first Proceedings states, "for the purposes of examining the natural history and antiquities of the county and its adjacent districts, *and of affording to such as were interested in the objects the opportunity of benefiting by mutual aid and co-operation*;" and while at its commencement it acted singly, and for a time alone, its interest and utility at length became known by the institution of similar clubs in the English counties adjoining, and within these few years by several very important ones springing up in other parts of England. The western counties of England have taken the most important lead; several societies, denominated "Field Clubs," have lately been instituted there. They are all formed after the model of the old Berwickshire Club, and profess to be bodies of *working* naturalists. It is the custom of their members to assemble during the summer months at the small and unambitious hostelries of their different counties, and, after breakfasting together, to transact the business of their societies and elect new members. This over, the Club divides into Geological, Botanical, and Entomological Sections, to which, as taste directs, the members attach themselves for the day. After a long walk they meet again at dinner, frequently at another village inn, eight or ten miles from the spot where they breakfasted. A homely repast is prepared, and generally discussed with much appetite. The remainder of the evening is devoted to reading scientific papers, examining the specimens collected during the day, and general conversation upon subjects of Natural History. A winter meeting is also held, where the sayings and doings of the past year are reviewed by the president in his address, new rules are passed, old ones altered, and the officers for the ensuing season elected. The elder of these west of England societies is the Cotteswold Club in Gloucestershire, under the presidency of T. B. Lloyd Baker, Esq. of Hardwicke Court, a gentleman who has much distinguished himself by his philanthropic endeavours to reform the young criminals and juvenile jail-birds of London, and other large towns. The Cotteswold Club is now in the tenth year of its existence. It was originally established by Sir T. Tancred, Bart., who for some years undertook the office of honorary secretary. On the departure of

Sir Thomas for New Zealand,* Professor James Buckman was elected. The first volume of the "Proceedings of the Cotteswold Naturalists' Club" was published in 1853. The papers by T. P. Wright, Esq., M.D., John Lycett, Esq., the Rev. P. B. Brodie, F.G.S., Professor James Buckman, and W. Hyett, Esq., are well known in the scientific world. "The Woolhope," the Herefordshire Naturalists' Field Club, was founded on the principle of the Cotteswold Society, in 1851, by the late Mackay Scobie, Esq., and the Rev. W. S. Symonds. Its progress has been steady and uninterrupted, and the members are doing much towards developing the geology and botany of their district; they are limited to fifty, and the list is full. A set of meteorological instruments have been purchased, and placed under the superintendence of Mr Hewett Wheatley. Their first volume of "Transactions" will be printed in June. The following gentlemen have filled the office of president:—1852, T. M. Lingwood, Esq., F.G.S., F.L.S.; 1853, The Rev. T. T. Lewis, the well-known Silurian geologist; 1854, The Rev. W. S. Symonds, F.G.S. The president for 1855 is the Rev. J. F. Crouch, F.L.S., late Fellow of Baliol College, Oxford.

The Malvern Naturalists' Field Club was established in 1852, and also consists of fifty members. The Rev. W. S. Symonds, F.G.S., has been president since the formation of the Club; the vice-president, the Rev. F. Dyson, and the honorary secretary, Mr W. Burrow, have also been re-elected. A local museum is being formed under the auspices of the members, at the house of the honorary secretary, and to which strangers visiting Malvern will be allowed access. The Club possesses a very fine collection of Malvern Silurian fossils. The "Transactions of the Malvern Club" will be published in June, when a great general meeting of the members of the three Clubs, joined by the members and council of the Natural History Society of Worcester, will be held at Malvern—Sir Roderick Murchison has promised to be in the Chair.

The Warwickshire Club, on the same principle, is just commencing work. President, the Rev. P. B. Brodie, distinguished by his work on Fossil Insects. The honorary secretary is the author of "Chronicles of a Clay Farm," Chandos Wren Hoskyns, Esq.

We hail, in the establishment of these societies, an extended knowledge in the local natural history of their counties, and heartily wish them success. It surely becomes an important feature in scientific history, when we find from 150 to 200 educated men engaged in such pursuits.

---

* Sir Thomas has just returned, and will, no doubt resume his usefulness.

Introductory Text-Book of Geology. By DAVID PAGE, F.G.S., Edinburgh and London. 1854. W. C., 12mo.

We heartily congratulate Mr Page on the production of a Geological Text-Book, which is at once clear and succinct, and in many respects well adapted to the beginner. In his preface he states, that " the utmost care has been taken to present a simple but accurate view of his subject;" and while we consider that this statement is justified in the main by the mode in which he has fulfilled his task, he has fallen into a few inaccuracies which we think it necessary to point out.

His arrangement of the " Silurian system" (p. 58) is perfectly correct, and he has placed the " Tilestones" at the top of the Ludlow series of rocks, for he is well aware that the " Tilestones" contain fossils eminently Silurian. *Orthoceras bullatum, Rhynconella nucula, Chonetes lata, Lingula cornea,* &c., &c. At the *base* of the " Tilestones" we find the " bone bed" of the Upper Ludlow rock; and there, with the remains of Silurian shells and crustaceans, for the first time in the geologic scale and the history of the planet, we meet with the fragments of fish. These fish are peculiar to the Silurian system, and the same ichthyolites have been found by Prof. Phillips in strata of the Upper Ludlow shales considerably *lower* than the " bone bed." " Each stratum," says Sir R. Murchison, speaking of the lowest member of the Old Red Sandstone and the fish beds of the Upper Ludlow, " is a fact confirmatory of the view of Agassiz, that those animals are very exact indicators of rocks."

Those who were present in the Geological Section at the British Association at Liverpool (September 1854), when Mr Page, with Sir R. Murchison's Siluria in his hand, was called to order by Sir Charles Lyell upon *this very point,* can hardly suppose that it was through *ignorance* our author penned the following passage in his recapitulation of the Old Red Sandstone :—" Characterized on its lower margin by strata containing the remains of fishes, and in this respect separated from the Silurian, *which is devoid of such fossils,* and defined, on its upper margin, by the rarity of that vegetation which enters so profusely into the composition of the carboniferous rocks, there can, in general, be no difficulty in determining the limits of the old red formation."

At page 63 he also " remarks that remains of fishes" must be " regarded as marking the *dawn* of the *Old Red Sandstone* epoch, rather than as belonging to the close of the Silurian;" and to which we reply that were mammalia found associated with the fossils of the Upper Chalk, he might argue, with equal truth, that the upper cretaceous deposit appertained to the epoch of the Eocene tertiaries ! This is not all : a similar mis-statement is applied to the *Silurian vegetation;* for (p. 66) we read that "the organic remains of the Old Red Sandstone" "furnish distinct evidence of *terrestrial vegetation;* as well as the *earliest traces of vegetable life* on our globe." Again (p. 136) we rise "from the *lowly sea-weeds of the Silurian strata;*" but Dr Hooker has determined fossil seeds from the Upper Ludlow rocks to belong to land plants allied to the Lycopodiaceæ.

With these exceptions Mr Page has compiled an excellent "Introductory Text-Book;" and we wish him every success; but, as it may be very extensively used, we are bound in duty to point out what we consider inaccurate.

Catalogue of the Birds in the Museum of the Honourable East India Company. Printed by order of the COURT OF DIRECTORS. Vol. I. London, 1854. 8vo.

There are many valuable Zoological collections in Great Britain, but most of them are comparatively useless from want of a catalogue or arranged list of the contents. Among these ranked the Museum of the Honourable East India Company, which has now set an example, by publishing the first part of the Catalogue of its Ornithological Collection. This museum has been long known as a valuable one, particularly in that department now being catalogued. Among its contents are the collections of drawings which have served as the foundations of many of the species described by Dr Latham, and which still continue as the sole authority for some of these. All the labours of Sir Stamford Raffles and Dr Horsfield are there, as well as the whole or part of the collections of General Hardwicke, Colonel Sykes, M'Clelland, Falconer, Hodgson, Strachey, Tytler, &c., &c.

The Catalogue is published under the superintendence of Dr Horsfield; but the actual labour of compiling it has devolved upon Mr F. Moore, the assistant curator, who has executed his work well. The systematic arrangement proposed by the late N. A. Vigors has been followed, and the volume now printed contains the *Raptores* of the collection, 103 species, and a portion of the *Incessores.* Extracts from various printed works of the donors of the specimens and drawings are introduced where they relate to the habits of the species.

CORRESPONDENCE.

Mr W. Mills, Missionary in Navigator Islands, writes us from Sydney, where he had gone on account of his health:—

"I am sorry to say the bird you were so anxious to get does not now exist in our island (Samoa), nor, so far as I can learn, in any of the other groups in the Pacific. I have procured all the Samoan birds except the Manu-Mea (Gnathodon), which has become all but extinct since the introduction of cats into the islands. I used every effort to get a specimen, but did not succeed. During a residence of eighteen years on the islands, I have only seen two Manu-meas."

"At all the islands east of Samoa very few birds are to be found, so that the Navigators' form quite a contrast. The missionary at the Harvey group supposes that the scarcity of birds there is occasioned from the destruction of their food by the frequent and dreadful hurricanes which they have."

Sydney, August 13, 1854.

Natal Geology. Extract of Letter from Dr P. C. SUTHERLAND to the late Professor E. FORBES, dated 5th June 1854.

"I send some specimens of copper ore from this colony. They occur between the junction of highly-contorted and almost vertically placed strata of the crystalline metamorphic rocks, with beds of non-fossiliferous sandstone, which not unfrequently pass into conglomerate on the one side and into shale on the other. The sandstone strata are nearly 1000 feet thick, are very rarely changed more than 10° to 15° from the horizontal line, and are frequently interstratified with beds of greenstone and basalt and other rocks of the trap series, which are often found decomposed into a grayish-yellow clay. In nearly the same geological position with the copper ore, masses of a species of talcose rock occur, and are found, although not with the copper, passing into rocks of a more steatose character, which in one or two instances showed an approach to a slightly fibrous structure, not unlike Asbestus. I send also specimens which appear to be olivine, from the same locality as the copper ore, but not near the gneiss. The presence of olivine among the granites found here may perhaps lead to giving it a place among rocks esteemed to be of earlier date than those which disturb the sandstone and other strata. It is very abundant among the gneiss strata of this colony. By a rough analysis of the copper ore, I found that some of the average specimens yielded 15 per cent. of the green carbonate (Malachite), or 8 per cent. of pure copper. * * * * *

"I send also specimens of calcareous nodules, which are found in many parts of Natal, and not unfrequently in sufficient quantity to be collected and burnt into lime. As they are not found except in the soil, which appears to have resulted from the decomposition of erupted rocks, it is probable they may be the nodules sometimes found in Amygdaloid. The sulphate of lime, which I have found they contain, may tend to protect them from the action of the carbonic acid present in the rain water which washed away the soil and left them exposed. There is also transmitted a specimen of crystalline limestone, occurring among the strata of the more quartzose gneiss rocks. It is taken from the immediate neighbourhood of an acidulous thermal water, which issues from among the rocks, at a temperature of 128°. A constant bubbling of carbonic acid escapes with the water, and imparts to it the acid properties it possesses, as shown by its action on litmus paper. Sulphur is deposited on the stones in the pools where the water cools down a few degrees, before it escapes into a large river close at hand, which sometimes overflows it. This river is called the Tugila. It flows from the Dooken bog, and follows a highly tortuous course. Where the mineral water occurs, it has made a section of the country to the depth of 3500 feet, and its fall from this part to the coast, over an extent of at least 40 miles, is not more than 500 feet. The strata are not contorted at the mineral water.

"I have to lament the loss we have sustained in the death of Dr Stanger as much, perhaps, as any person in the colony. He contemplated a grand geological examination, which would have been carried on in connection with the survey on which he was preparing to enter."*

Himalayan Geology. Extract of Letter from T. OLDHAM, Esq., to the late Professor E. FORBES, dated January 19, 1854.

"Coming down from Darjiling last year I visited a locality where coal was said to occur at the base of the outer ranges of the Himalayas, and though I did not find coal, I found a very interesting series of sandstones and clays, at least 4000 to 5000 feet thick, with numerous imbedded stems of trees, palms, &c., and in one place with leaf-beds—no animal fossils—all dipping at considerable angles into the range of hills, and apparently cut off by a great fault from the gneiss of the great central portion there. There was no trace of the great nummulite group here, but I believe this thick series is the true representative in this part of the Himalayan range of the Sewalik group in the north-west. I did not find, nor did I hear of there having ever been found, any of the large fossils there discovered. But this is the case in many other places in the prolongation of this great group.

---

* [We trust that Dr Sutherland will send further communications on the geology of Natal and of the copper district.—ED. *Phil. Jour.*]

I fancy I have worked out pretty well that the great coal deposits of Bengal (Burdwan, &c.) are of the older oolitic era, (to compare Indian with European groups). We found fossils last season, which, as far as I can see, are identical with those from Cutch, described by Morris in the Transactions of the Geological Society, vol. v. (Ptilophylla), in the same beds containing Vertebraria and the common coal-series plants of Bengal. Now, though vegetable remains are but poor evidence after all, they are something. And as the Cutch ones are truly oolitic, I am inclined to refer the whole group to that period."

Gutta Percha in India. Extract of a letter from Dr Hugh Cleghorn, Madras, to Professor Balfour, dated 13th January 1855.

"Three days ago, my friend Colonel Cotton, of the Madras Engineers, sent me a piece of Gutta Percha from the Wynaad, with a twig of the tree producing it, which is a true *Isonandra.* I have on the table—both the gum-elastic and the branchlet—abundant proof of the important discovery. It is believed that the tree grows abundantly in Malabar. I have requested that a diligent search should be made. As telegraphic lines stretch across our Peninsula, the importance of the discovery can scarcely be overrated, now that the forests of Singapore are wellnigh exhausted. The government will take means to preserve a wholesale destruction in the present instance, by making the forest a royalty, or at all events placing the trees under strict conservancy. I await with deep interest further intelligence from the distinguished engineer as to the extent of the gutta percha forests."

PROCEEDINGS OF SOCIETIES.

Royal Society of Edinburgh.

Tuesday, 2d January 1855. Right Rev. Bishop Terrot, in the Chair.

The following communications were read:—

1. *Notes on some of the Buddhist Opinions and Monuments of Asia, compared with the Symbols on the Ancient Sculptured "Standing Stones" of Scotland.* By Thomas A. Wise, M.D.

The general identity, in idea and design, of the ancient monuments of southern and western Europe with those of Hindostan, was shown and illustrated by drawings of cairns, barrows, kist-vaens, cromlechs, circles of stones, and obelisks, or, as they are frequently called, standing stones, as found in both regions. The connection between the inhabitants of these regions was further shown by the physical conformation of the races, by the similarity of many of their manners, customs, and observances, and by the decided and extensive affinity of the Celtic, and other languages of western Europe, with the Sanscrit. The early connection which thus appears to have existed was shown to indicate a line of inquiry, by following which much of the obscurity, resting over the earliest monuments and history of western Europe, may be cleared away. In particular, reasons were adduced for believing that the doctrines of Buddhism, originating in Asia, at a period when some intercourse was still maintained between the cognate, but widely separated races, were carried westward by missionaries, who, finding the people unprovided with a written language, had recourse to symbols, already used in the East, to express their fundamental doctrines. The deity or spirit (Buddha) was designated, as in India, by a wheel or circle; inorganic matter (Dharma) by another circle, or by a monogram, formed of the initial letters of the elements; and organic matter (Sanga) by some embryotic form of animal or vegetable life, or by a circle or an imperfect crescent. The symbol of three single circles is found in both regions: This triad is found in India in the temple of Ellora, and other Buddhist temples, and in Scotland on the Kineller stone. In the progress of advancement of the arts, these simple forms of symbols were changed for temples, and idols were added by the rich and powerful Buddhists of Asia.

Among the ruder and more ignorant inhabitants of Scotland, the arrangement of the symbols required to be altered, to suit the people for whom they were intended: Spirit and Matter continued to be represented by two circles, but connected by a belt, and crossed by a bar uniting the extremities of two sceptres, to indicate the supreme power of these (according to the Buddhist creed) co-ordinate and all originating principles; while organised matter was represented by a crescent, flower, a dog-like embryo, or some other rude representation of life.

The modifications of the serpent figure, and the Buddhist cross or sacred labyrinth, as symbols of the spiritual deity; and the occurrence of lions, camels, centaurs, with the honour paid to trees, &c., on the ancient sculptured obelisks of Scotland, were also adduced as proofs of an oriental origin, or connection.

Reasons were given for the number of these stones in that part of Scotland forming the ancient Pictish kingdom; of which the inhabitants, after a temporary profession of Christianity seemed to have declined from the faith.

2. *Note on the extent of our knowledge respecting the Moon's Surface.*
By Professor C. Piazzi Smyth.

Taking advantage of the special attention paid at present to certain astronomical disquisitions, the author called attention to a particular point connected with the moon, which was first stated by the author of "The Plurality of Worlds," and then made by him to prove that the moon must be uninhabited, and thence to lead to the conclusion that all the other planets were uninhabited also. This point was, that "observations having been made on the moon abundantly sufficient to detect the change caused by the growth of such cities as Manchester and Birmingham, no such changes having been perceived, the theory of non-habitation may be indulged in."

But after having indicated the sort of appearance that those collections of human habitations would make when transferred to the moon, Professor Smyth proceeded to show that the registered and published observations of the moon are by no means sufficiently accurate to be used to test this question: and that they do show changes, and often to a far greater amount than the mere building of a lunar Manchester would occasion, but such changes bear the impress of error of observation. More powerfully still was this brought out, on comparing even the best of the published documents with some manuscript drawings of the Mare Crisium in the moon, recently made at the Edinburgh Observatory; and the author hoped that this statement of the imperfection of existing maps would lead to observers generally applying themselves to improve this important and interesting field of astronomy.

3. *On the Interest strictly Chargeable for Short Periods of Time.* By the Rev. Professor Kelland.

Monday, 15th January 1855. Dr Traill, Curator of the Library, in the Chair.

The following Communications were read:—

1. *On the Ethers and Amides of Meconic and Comenic Acids.* By Henry How, Esq. Communicated by Dr Anderson.

This paper appears at page 212 of the present number of this Journal.

2. *On the Result of a Revision of the British Association Catalogue of Stars at the Madras Observatory.* By Captain W. S. Jacob. Communicated by Professor C. Piazzi Smyth.

See page 206 of the present number of this Journal.

3. *Notice of Ancient Glacier Moraines in the Parishes of Strachur and Kilmun, Argyleshire.* By Charles Maclaren, F.R.S.E.

See page 189 of the present number of this Journal.

Monday, 5th February 1855. The Right Rev. Bishop Terrot in the Chair.

The following Communications were read:—

1. *On the Properties of the Ordeal Bean of Old Calabar, Western Africa.* By Dr Christison.

In various parts of Western Africa it appears to be the practice to subject to the ordeal by poison persons who come under suspicion of having committed heinous crimes. On the banks of the Gambia river the poison used for the purpose is the bark of a leguminous tree, the *Fillæa suaveolens* of MM. Guillemin and Perottet. In the neighbourhood of Sierra Leone it is the *Erythrophleum guinëense*, which some botanists have considered identical with the former species. On the Congo river, Captain Tuckey found that either this species, or an allied species of the same genus, was in constant use for the same purpose. These barks, when their active constituents are swallowed in the form of infusion, sometimes cause vomiting; and then the accused recovers, and in that case is pronounced innocent. More generally the poison is retained; and then the evidence of guilt is at the same time condemnation and punishment; for death speedily ensues.

In the district of Old Calabar, the poison used for the trial by ordeal is a bean, called *Eséré*, which seems to possess extraordinary energy and very peculiar properties. It has been lately made known to the missionaries sent by the United Presbyterian Church in Scotland to the native tribes of Calabar; and to the Rev. Mr Waddell, one of these gentlemen, the author was chiefly indebted for the materials for his experiments, as well as for information as to its effects on man. According to what the missionaries often saw, this poison is one of great energy, as it sometimes proves fatal in half an hour, and a single bean has proved sufficient to occasion death. None recover who do not vomit it. The greater number perish. On one occasion forty individuals were subjected to trial, when a chief died in suspicious circumstances, and only two recovered.

The author found the bean to present generally the characters of a *Dolichos*. It has been grown at his request both by Professor Syme and at the Botanic Garden by Mr M'Nab; and it proves to be a perennial leguminous creeper, resembling a Dolichos, but it has not yet flowered. The seed weighs about forty or fifty grains. It is neither bitter, nor aromatic, nor hot, and differs little in taste from a haricot bean. Alcohol removes its active constituent, in the form of an extractiform matter, amounting to 2·7 per cent. of the seed. The author could not obtain an alkaloid from it by any of the simpler processes for detecting vegetable alkaloids.

By experiments on animals, and from observation of its effects on himself, the ordeal bean has a double action on the animal body: it paralyses the heart's action, and it suspends the power of the will over the muscles, causing paralysis. It is a potent poison, for twelve grains caused severe symptoms in his own person, although the poison was promptly evacuated by vomiting, excited by hot water. The alcoholic extract has the same effect and action with the seed itself.

2. *Experiments on the Blood, showing the effect of a few Therapeutic Agents on that Fluid in a state of Health and of Disease.* By James Stark, M.D., F.R.C.P.

3. *Extracts from a Letter from E. Blackwell, Esq., Chamouni, containing Observations on the Movement of Glaciers in Winter.* Communicated by Professor Forbes.

The accessibility of the glaciers, even up to a considerable height, is at this season a question of mere physical force. I have made within the last few days two excursions into the region of perpetual snow. The first of these was on the 6th of January, and was to the summit of the glacier of Blatière, several hundred feet above the point where I had noted the line of the nèvè in September and October; the second was on the 13th, when I succeeded in reaching the junction of the glaciers of Bossons and Tacconaz, near the Grands Moulets. This junction is exactly at the commencement of the nèvè, as I remarked between the months of August and October, on six different occasions, when I passed there on my way to and from Mont Blanc, the Dôme du Gouter, &c. In both these expeditions I was struck by the excessive power of the sun; the greater apparent warmth, even in the shade, as compared to the valley of Chamouni; and the sudden chill which followed sunset. There was also much less snow at these heights than in the valley, and I have no hesitation in saying that in winter very little snow falls upon the higher summits. The snow-falls in the valley are *invariably* brought by a low creeping fog, which comes up from Lallanches. It seldom overtops the Col de Voza, and the Aiguilles appear bright and sunny in the gaps of the cloud. It is in spring and autumn that these high peaks are powdered by every storm; *now* the dispersing clouds leave them as dark as before they gathered. I fancy this winter is unusually cold; every one is crying out, and complaining that the potatoes are frozen in deep cellars. I have seen Reaumur's thermometer at −25° at 5½ in the afternoon, and I think it may reasonably be supposed that it may have fallen to −30° during the night; wine has frozen on my table before a fire. In the woods the trees crack with the intense frost and there is from 2½ to 3 feet of snow in the valley without drifts; on the glacier of Blatière there is only from 1 to 2 feet.

In spite of all this cold the glaciers advance steadily. The glacier de Blatière, terminating above the line of trees, pushes its moraine in front of it, and seems to be on the increase. Now this is a very *shallow* glacier, and, as I have said, covered with but little snow. Is it possible that infiltrated water can have any action whatever under such circumstances?

I will here state a few results of careful observation, and I hope that, even should they appear strange, you will yet consider them worthy of confidence. I have no theodolite, but I have a prismatic compass, and will take the bearings of various points from my stations should you deem it advisable.

The torrent of Bossons has been quite dry ever since the beginning of November, and I have profited by this circumstance to endeavour to determine the motion of the ice within the vault, nearly in contact with the ground. I believe it is usually supposed that the reason why the termination of a glacier seems stationary in summer, is that there the waste predominates over the supply. It seemed to me therefore, that in winter, when there is actually no waste—the torrent being perfectly dry, and its subglacial bed even *dusty*—the end of the glacier ought to be thrust forward into the valley by the pressure behind. I accordingly with some little difficulty, fixed a station on the ridge or back of the glacier, near the lower extremity; the result is, that *the ice there is nearly stationary*. This is doubtless a clue to the assertions of some authors, 'that the glacier is stationary in winter;'—they only looked at *the end*. What becomes, then, of the ice continually descending from above? Does it not go to thicken the whole mass, accumulating behind the more rigid portion below, as water behind a dam? I have no space to add more at present, but will write again if I have your approval of my proceedings. Meanwhile I have fixed (yesterday) an intermediate station, for the purpose of determining *where* this comparative immobility begins. I have noted my observations, and kept a register of weather, &c. I give one observation to show the difference between the middle and lower glaciers:—

From December 28 to January 11—14 days.

Middle glacier (somewhat above where it is usually crossed).

Centre, 14 ft. 7 in. (fourteen feet, seven inches).
Side, 11 ft. 6 in. (eleven feet, six inches).

Lower glacier in the same period.

Ridge, 1 ft. 7 in. (one foot, seven inches).
Interior of vault, 0 ft. 2 in. (two inches).

Observations on Mr Blackwell's Letter. By Professor FORBES.

The cold described (−25° to −30° of Reamur−24½° to −35½° of Fahrenheit)—appears so excessive as to be unlikely; I have therefore written to enquire if the thermometer could be depended on.

It is highly satisfactory that the superficial velocity of the glacier of Bossons—about a foot in twenty-four hours—coincides closely with the measurements of my guide, Auguste Balmat, some years since, on the same glacier, at the same season.

With respect to the ice of the glacier of Blatière, which is above the level of trees—probably at least 7000 feet above the sea—being still in motion, it merely confirms the deductions long ago made by me as to the continuity of glacier motion even in winter. And as to the apparent paradox of water remaining uncongealed in the fissures of the ice at this season, though I have nowhere affirmed the presence of liquid water to be a *sine qua non* to the plastic motion of glaciers, it would be difficult to assert positively that it is everywhere frozen in the heart of a glacier even in the depth of winter. Heat, we know, penetrates a glacier (up to 32° and no further), not only by conduction, but much more rapidly by the percolation of water; but cold penetrates *solely* by conduction, and that according to the same law as in solid earth, though it may be more rapidly. Now, it is known that at a depth of 24 or 25 feet in the ground the greatest summer heat has only arrived at Christmas. A similar retardation in the effects of cold must occur in glaciers. Not a particle of water detained in the capillary fissures can be solidified until its latent heat has been withdrawn.

The contrast the writer draws between the glaciers of Blatière and Bossons, the latter of which is some thousand feet lower in point of level, is curious and instructive. The former, he says, appears the more active, and is pushing forwards its *moraine;* whilst the latter, at its lower extremity, and in contact with the ground, is scarcely moving at all.

There is nothing of which we know less than the cause of this seemingly capricious advance and retreat of the extremities of glaciers at the same time, and under, seemingly, the same circumstances.

In the present case, I will only mention as a *possible* explanation, that the glacier of Blatière probably possesses a continuous slope, from its middle and higher region down to its lower extremity. But the Bossons, after its steep descent from Mont Blanc, proceeds a long way on a comparatively level embankment, which at an early period it cast up of its own debris, and in which it has dug itself a hollow bed in which it nestles. The angular slope of the bottom in contact with the soil is very probably much less than in the case of the glacier of Blatière. Now, when winter has dried up the percolating water, the viscosity of the mass may be insufficient to drag it over the less slope although it carries it over the greater. That the motion of the ice close to the ground should be nearly nothing, whilst the more superficial part of the glacier over-rides it by its plasticity, is as a separate fact quite in accordance both with theory and previous observation.

But as the *snout*, or lower end of the glacier of Bossons, is almost stationary, whilst the middle region is moving at the rate of a foot a day, Mr Blackwell very pertinently asks, " What becomes, then, of the ice continually descending from above? Does it not go to thicken the whole mas, accumulating behind the more rigid portion below, as water behind a dam?" I answer, undoubtedly; and he will find this explanation given ten years ago in my *Travels in the Alps*, (2d edit., p. 386.) Speaking of the superficial waste of the glaciers in summer and autumn, and the manner in which it is repaired before the ensuing spring, I there observed, " The main cause of the restoration of the surface is the diminished fluidity of the glacier in cold weather, which retards (as we know) the motion of all its parts, but especially of those parts which move most rapidly in summer. The dis-proportion of velocity throughout the length and breadth of the glacier is therefore less, the ice more pressed together, and less drawn asunder; the crevases are consolidated, while the increased friction and viscosity causes the whole to swell, and especially the inferior parts, which are the most wasted."—(See also *Seventh Letter on Glaciers*, p. 435 of Appendix to the same work.)

Monday, 19th February 1855. James Tod, Esq., in the Chair.

The following Communications were read:—

1. *On the Mechanical Action of Heat*:—Supplement to the first Six Sections, and Section Seventh. By W. J. Macquorn Rankine, Esq., C.E.

2. *On an Inaccuracy (having its greatest value about 1″) in the usual method of computing the Moon's Parallax.* By Edward Sang, Esq.

When, as in the usual operation, the moon's obscured zenith distance is corrected for the effects of atmospheric refraction, the zenith distance so obtained is that of the rectilineal part of the ray of light between the planet and the upper surface of the air; and on applying that correction as at the observatory, we do not obtain the direction of the moon as it would have been seen if there had been no atmosphere, but that of a line drawn parallel to the first part of the ray, and therefore passing below the moon. The true direction of a straight line drawn from the observer to the planet, must differ from this direction by the angle which the curved part of the ray subtends at the moon's centre; and the neglect of this angle may cause a sensible error in estimating the parallax.

It is a well-known property of refraction by concentric strata, that the perpendiculars let fall from the centre of curvature upon the tangent to the path of light are inversely proportional to the indices of refraction of the medium at the two points of contact.

From this property it very easily follows that the sine of the true parallax is obtained by multiplying the sine of the horizontal parallax by the sine of the observed zenith distance, and by the index of refraction of the air at the observatory.

And if the horizontal parallax given in the almanac, instead of being the half angle under which the earth would have been seen from the moon if there had been no atmosphere, had been the true horizontal parallax, or half the angle which, in the actual state of things, the earth does subtend at the moon,—the true method of computing the parallax would only differ from the common one in the use of the uncorrected instead of the corrected zenith distance.

In the common formula, the multiplier is the sine of the zenith distance corrected for refraction; in the true formula, it is the sine of the uncorrected zenith distance, multiplied by the index of refraction of the air.

For the purpose of obtaining the maximum error of the common formula, it is observed that when the moon is in the horizon, the zenith distances being nearly 30°, have their sines sensibly equal to each other, and that then the true multiplier must exceed the usual one in the ratio of 3405 to 3404,—this ratio being the index of refraction of air in its mean state; wherefore at the horizon the parallax, as usually computed, must fall short of the true parallax by one 3404th part of itself.

This ratio holds good for all planets; and it is only in the case of the moon that the error becomes sensible, being then almost exactly one second of arc.

Monday, 5th March. The Right Rev. Bishop TERROT, in the Chair.

The following Communications were read:—

1. *On Annelid Tracks in the Equivalents of the Millstone Grits in the south-west of the County of Clare.* By Professor HARKNESS.

See page 278 of the present number of this Journal.

2. *On Superposition.* By Professor KELLAND.

The object of this paper was to defend the method of demonstration employed by Euclid from some of the charges which have been at various times brought against it. It particular, it was shown, that the method is not deficient in variety of demonstration of the same fact. This position was illustrated by the exhibition of twelve totally different demonstrations of the problem, "To cut three-fourths of a square into four pieces, which shall form a square."

3. *On the Colouring Matter of the Rottlera Tinctoria.* By Dr ANDERSON.
See page 296 of the present number of this Journal.

Monday, 19th March. Colonel MADDEN, Councillor, in the Chair.

The following Communications were read:—

1. *Experiments on Colour as perceived by the Eye, with Remarks on Colour-Blindness.* By JAMES CLERK MAXWELL, Esq., B.A., Trinity College, Cambridge. Communicated by Professor GREGORY.

These experiments were made with the view of ascertaining and registering the judgments of the eye, with respect to colours, and then, by a comparison of the results with each other, by means of a graphical construction, testing the accuracy of that theory of the vision of colour, which analyses the colour-sensation into three elements, while it recognises no such triple division in the nature of light, before it reaches the eye.

The method of experimenting consisted in placing before the eye of the observer two tints, produced by the rapid rotation of a system of discs of coloured paper, arranged so that the proportions of each of the component colours could be changed at pleasure. The apparatus used was a simple top, consisting of a circular plate on which the coloured discs were placed, and a vertical axis. The discs consisted of paper painted with the unmixed colours used in the arts. Each disc was slit along a radius from centre to circumference, so that several could be interlaced, so as to leave exposed a sector of each. The larger discs, about 3 inches diameter, were first combined and placed on the disc, and the smaller, about 1¾ inches diameter above them, so as to leave a broad ring of the larger discs visible.

When the top was spun the observer could compare the resulting tint of the outer and inner circles, and by repeated adjustment, perfect identity of colour could be obtained. The proportions of each colour were then ascertained, by reading off on the circumference of the top, which was divided into 100 parts. As an example, it was found on one occasion, that,—

$$\left.\begin{array}{r} \cdot37 \text{ Vermilion,} \\ +\cdot27 \text{ Ultramarine,} \\ +\cdot34 \text{ Emerald green,} \end{array}\right\} = \left\{\begin{array}{l} \cdot28 \text{ White} \\ +\cdot72 \text{ Black} \end{array}\right.$$

By experiments on various individuals, it was found (1.) that a good eye could be depended upon within two of these divisions, or hundredths at most, and that by repetition of experiments, the *average* result might be made much more accurate.

(2.) That the difference of the results of experiments on different individuals was insensible, provided the light used remained the same.

(3.) That when different kinds of light were used, or when the resultant tints were examined with coloured glasses, the results were totally changed.

It follows from this that the cause of the equality of the resulting tints is not a true optical identity of the light received by the eye, but must be sought for in the constitution of the sense of sight. The materials for this inquiry are to be found in the equations of colour, of which the above is an example, and these are to be viewed in the light of Young's theory of a threefold sensation of colour.

The first consequence of this theory is, that between any *four* colours an equation can be found, and this is confirmed by experiment.

The second is, that from two equations containing different colours a third may be obtained by the ordinary rules, and that this also will agree with experiment. This also was found to be true by experiments at Cambridge, which include every combination of five colours.

A graphical method was then described, by which, after fixing arbitrarily the positions of three standard colours, that of any other colour could be obtained by experiments in which it was made to form a neutral grey along with two of the standard colours. In the diagram so formed, the position of any compound tint is the centre of gravity of the colours of which it is composed, their *masses* being determined from the equation, and the resultant *mass* of colour being the sum of the component *masses*. The colour-equations represent the fact that the same tint may be produced by two different combinations. This diagram is similar to those which have been given by Meyer, Hay, and Professor J. D. Forbes, as the results of mixing colours. It is identical with that proposed by Young, and figured in his *Lectures on Natural Philosophy*. The original conception, however, seems to be due to Newton, who gives the complete theory, with an indication of a construction in his Optics.

The success of this method depends entirely on the truth of the supposition that there are three elements of colour as seen by the eye, every ray of the spectrum being capable of exciting all three sensations, though in different proportions. It is at present impossible to define the colours appropriate to these sensations, as they cannot be excited separately. But it appears probable that the phenomena of colour-blindness are due to the absence of one of these elementary sensations, and, if so, a comparison of colour-blind with ordinary vision will show the relation of the absent sensation to those with which we are familiar.

A method was then described, by which one observation by a colour-blind eye was made to determine a certain point representing the absent sensation, which thus appears to be a red approaching to crimson. The results of this hypothesis were calculated in the form of "equations of colour-blindness" between colours which seem to defective eyes identical. These equations were compared with those previously determined from the testimony of two colour-blind, but accurate observers, and found to agree with remarkable precision, rarely differing by more than 0·02 in any colour. The effect of red and green glasses on the colour-blind was then described, and a pair of spectacles having one eye red and the other green was proposed as an assistance to them in detecting doubtful colours.

Observations on Mr Maxwell's Paper. By Dr G. Wilson.[1]

I greatly regret that indisposition will not allow me to attend the meeting of the Royal Society this evening, especially after Mr Clerk Maxwell has had the kindness to send me his MS. I should have liked to express my admiration of his beautifully simple device for testing, quantitatively as well as qualitatively, colour-vision, and of referring to the value of his results. Now that railway managers are fully alive to the necessity of ascertaining the quality of colour-vision of their servants, both for the sake of excluding the colour-blind and of certifying the acuteness of visual perception of those who are to handle and interpret railway-signals, the colour-top will prove of great service in determining those points. Our regimental, naval, and hospital surgeons also, but especially those on the recruiting service, will have the opportunity (at least when the pressure of war is over) of employing this instrument as a means of accumulating important data in reference to the perception of colours. But on this I need not enlarge.

In the cursory perusal which I have been able to give Mr C. Maxwell's paper, the points which have struck me most have been the following, and, if agreeable to the Society, I should be glad if you would communicate them to it :—

1. It is satisfactory to find the author, while working independently, and pursuing a mode of research peculiar to himself, reach the conclusion concerning colour-blindness, that it is the habitual vision of *two* colours, blue and yellow, whilst normal vision is the habitual perception of *three*, blue, yellow, and red. Sir John Herschel, it now appears (viz. since the publication of Dalton's Life) proposed, more than twenty years ago, to distinguish colour-blindness as dichromic vision. I have urged the same conclusion as making it vain to expect that more than one-third of the phrenological organ of Colour (supposing such an organ to exist) should be conspicuously wanting in the colour-blind, and as rendering it hopeless to employ more than two-coloured signals, if those who are colour-blind are allowed to act as signal-men. But though colour-blindness may be conveniently referred to as identical with two-colour vision, it seems questionable whether this is strictly accurate. The sensation of red does not appear to be altogether absent from the colour-blind. On the other hand, many of them distinguish red *at times* from blue and yellow, as well as from green, and, so far as one may judge from their language, their sensation of red is *then* the same as ours. Mr Maxwell's experiments with the Cambridge students are not at variance with this being the case. They show the great liability to mistake red which unquestionably characterizes the colour-blind; but the latter should *never* see red if their eyes are devoid of the nervous apparatus essential to the red sensation. I am inclined to think that, with very few exceptions, the vision of every one is trichromic, though for practically useful purposes it is only dichromic in the colour-blind.

2. It seems to me exceedingly doubtful whether we sufficiently *fully* define colour-blindness, even in reference to the utilitarian perception of colours, by regarding it as equivalent to the non-perception of red. All the records of colour-blind cases appear to show that the darker shades of *all* colours are confounded with each other and with black, and the lighter shades with each other and with white, in circumstances where the defect of white light on the one hand, and the excess of it on the other, do not

---

[1] In a Letter to Professor Gregory.

prevent a normal eye from distinguishing the accompanying colour from the blackness or whiteness which tends to extinguish it.

If this be the case (and Mr Maxwell's method and apparatus would serve admirably for testing the truth of the belief), then a colour-blind eye is not a normal eye in all but the perception of red, nor can colour-blindness be properly defined as "anerythric or no-red vision." A colour-blind eye is, I apprehend, abnormal, in its perception of certain at least of the tints and shades of all colours, and this so far justifies the phrenological hypothesis of a diminution of the entire organ of Colour (if there be such an organ at all) in the colour-blind, and is to myself one of the strongest justifications of the use of the word colour-blindness, which, however, is of Sir David Brewster's coining, not of mine.

3. The question why green should be mistaken for red remains still a puzzle, but I cannot enter into the discussion of this question at present. I hope to bring it before the Society again.

4. Mr C. Maxwell's spectacles for the colour-blind introduce a new and important feature into the construction of optical aids for their defects. In the many previous proposals to use coloured glasses, the colour-blind person had no means of deciding what colour of glass he was at the moment using, and might fancy himself looking through a red glass when he was using a green.

But by placing red in one eye of the spectacles, and green in the other, and making it simply a question which, used *singly*, renders a colour known to be *either* red *or* green *brighter*, the decision of the true nature of the colour resolves itself into brightness under right eye, *versus* brightness under left eye, supposing the spectacles to be made, as they generally are in England, so as to bridge the nose only in one way. The foreign double-bridged spectacles would defeat the end in view.

5. In conclusion, I would, through you, beg Mr Maxwell not to confine himself to sharply defined cases of colour-blindness, but to extend his beautiful method of inquiry to the less attractive but more common cases of uncertainty as to all colours, which we may anticipate he will also bring under law.

Observations on Mr Maxwell's Paper. By Professor J. D. Forbes.

I do not know whether you advert at all to the history of experiments on the mixing of colours, but I may mention that I find by my register, that my chief experiments were made on the 4th and 12th January 1849; and amongst these results I find "yellow 100°, blue 120°, white 140°, produce a quite neutral gray like black 180°, white 180°." "Yellow and blue only, equal, produce a yellow grey or citrine—*never green.*" [The yellow was gamboge.]

On the 1st March 1849, I have the following entry: "Examining the red, yellow, and blue papers by the colours they reflect in a dark room, when a narrow slip of each was strongly illuminated by the sun, and the light examined (not in the plane of reflection) by a prism, the colours appear very complex indeed. Both the red and yellow reflect almost every colour of the spectrum. The blue seems purest, but very decidedly violet or tinged with red.

2. *Notice of the Occurrence of British newer Pliocene Shells in the Arctic Seas and of Tertiary Plants in Greenland.* In a letter from Dr Scoular of Dublin. Communicated by James Smith, Esq., of Jordanhill.

Dr Scoular to Mr Smith.

"I have lately had the opportunity of examining a series of fossils from high Arctic latitudes, brought home by Captain M'Lintock, R.N. The series in one sense is extensive, as there are silurian and oolitic shells, and also other fossils of the tertiary epoch. Among these last there are some things which I am sure will be of interest to you. Among the specimens are some recent and living shells from Baring's Island, of which I will send you a list when I determine the species. In the meantime I may state with full confidence that the variety called Mya udevallensis, so common a fossil with us and in Sweden is still a living species at Baring's Island. The truncated form of the shell, and the palliar impressions, are those of the Mya udevallensis, and not those of the modern M. truncata. On the truth of this you may fully rely, and also that the shells were taken with the animal in them.

"In the collection there are also some fossil plants from Greenland. They are not, however, carboniferous, but, to my surprise, tertiary, and of the same character as those of the Mull formation. I could not find any difference between them and the fossil leaves from Mull, but I cannot at present command the paper of the Duke of Argyle; however, I have not the smallest doubt of the identity of the formation and species."

Royal Physical Society.

Wednesday, November 22, 1854. HUGH MILLER, Esq., P., in the Chair.

1. Mr HUGH MILLER delivered an opening address "On the Fossiliferous Deposits of Scotland." (This has been published as a separate pamphlet.)

2. *On a curious habit stated to have been observed in one of the Woodpeckers in California.* By ANDREW MURRAY, Esq.

In this communication, Mr Murray stated, he had received information on the habits of one of the Californian woodpeckers, which appeared to him both sufficiently new and interesting to be worthy of being made generally known to naturalists; and although the information is imperfect, and may possibly turn out to be incorrect, he was bold enough to communicate it to the Society. The statement is, that a particular Woodpecker in California lays up a store of acorns in autumn for its spring consumption, and does so by hammering out small holes in the bark of trees, into each of which it places an acorn. His informant was his brother, Mr William Murray, whose botanical tastes may be probably known to some of the members of the Society. He resides at San Francisco; but when home on a visit last year, he mentioned the habit of the woodpecker which has just been related. Shortly after his return to California, he received from him the piece of bored bark, which he exhibited to the Society, and at the same time communicated the following information which he had picked up. He says,—"I was talking to Simson the other day about the curious custom the woodpeckers here have of boring holes in the bark and storing them with acorns, when I mentioned that I had told you of it, and that you had refused to credit the fact, not of the acorns being there, but of their being put there by woodpeckers, because I was unable to say I had *seen* them put there. 'Well,' said he, 'you can tell him that I've seen them. I have seen them bore the holes, put in the acorns, and hammer them well in, and I've seen them take them out again in spring;' and he went on to tell me, that on one occasion, in the time of the great flood (some years ago), he had witnessed an amusing scene among them. His party were camped on a kind of island that had been left dry; and having nothing better to do, watched the operations of these birds. There were six or eight of them at work on a tree, in which there was a squirrel, who had made his house in a hollow at the root of a branch. The squirrel would pop out his head and look at them; and the moment the coast was clear, he would run out and scratch away at these things, and tear away the bark; and when the birds would see him, they would all attack him, and he would run like lightning down the tree, and up the other side and into his hole again, and then peep out and watch another chance to do the same, evidently having great fun. This continued for about three days, till at last one of the party knocked the squirrel's head off with a rifle-ball, and rid them of their persecutor." In a subsequent letter his brother gives the following additional information. He says—" Newland, a Scotchman, told him he had often seen the woodpecker storing the acorns, and that it was a black bird with a red head; but Simson, he said, would introduce me to Dr Trask (author of the geological report herewith sent), and that he would be able to say positively. The Doctor stated that the provident woodpecker is the black one with the red head and yellow throat, that he had observed them repeatedly; and further asserted that they eat acorns, and that he had seen them do it. In confirmation of the possibility at least of their being vegetable feeders, Simson tells me that in the western country the farmers frequently clear the woods by cutting the communication of the bark of the trees, and that, where that is done, these red-headed woodpeckers appear in the clearings in perfect swarms, and destroy apples and peaches in these districts to such an extent that it is impossible to have any fruit. I do not know whether they eat the acorns or the grub that may be in them, but it is most certain that they bore holes in the bark, and hammer in the acorns so firmly that you can hardly pick them out again, and afterwards break them open, and eat something that is within the shell. The native Californians are so well acquainted with the fact, that they say when the woodpeckers commence early, it is a sign that we shall have a severe winter. They keep boring the holes all the summer, and are all ready for harvest when the acorns are ripe." My brother adds that Mr Simson came across Mexico with John Audubon (he presumed the son), who watched them, stuffed their skins, and knows all about them. They first observed these acorn deposits in Chihuahua. Mr Murray was inclined to think that the evidence contained in these letters would be sufficient to satisfy the Society, as it had done himself, that there is good ground for believing that *bona fide* acorn deposits are in California stored up for future consumption by a woodpecker.

3. *Notice of the Lepidopterous captures near Edinburgh, during the past Season.* By WM. H. LOWE, M.D.

Dr Lowe having been appointed Convener of the Entomological Committee at the last winter meeting of the Society, said, he thought that, although from the small number of entomologists in Edinburgh, and those for the most part engaged in active professions, little had been accomplished during the past summer, still he had several species of *Lepidoptera* to bring forward as new to the list published by him and Mr R. F. Logan in 1852. As his own captures, he mentioned *Trachea piniperda* (two specimens), *Micropteryx unimaculella, Lampronia quadripunctella, Peronea Hastiana, Jinea Zinkenii.* To these he had to add, *Pterophorus acanthodactylus*, 1851, *Argynnis selene*, 1853, *Satyrus dabus, Hepialus yelleda, Cabera exanthemaria, Euthemonia plantaginis, Zanthia rufina, Dosithea reversaria,* all which were owing to the industry of Mr Andrew Wilson of this city, and with the exception of *Cabera exanthemaria,* which had been previously taken by Mr Peter Fairbairn, as well as by Dr Lowe, were additions to the insects of this district. Dr L. also noticed *Coccyx strobilana*, which had been taken in a greenhouse at Newington, and which was traced to a basket of fir cones sent to Edinburgh by Mrs Scott of Gala. Among other insects also observed, and taken this year were *Macaria lituraria, Leucania lithargyria, Spœlotis cataleuca, Agrotis obelisca, A. putris, Caradrina morpheus, Hadena adusta, &c.* There was also a fine series of *Dosithea scutularia*, bred from caterpillars, and which, in that early stage of development, had been frozen hard, and left to thaw in the ordinary way, but which had, nevertheless, produced beautiful specimens. Another brood of caterpillars of a different genus, which had been similarly exposed, had entirely perished. The results of a day's ramble in Castle Eden Dean, in the county of Durham, were included in the insects brought before the Society. Among them were *Dosithea blomeri, Pyraustra Punicealis, Stigmonota trauniana, &c.*

Mr R. F. Logan exhibited specimens of *Bombycia viminalis*, bred from larvae found in June on a dwarf sallow on the Pentlands; also a male *Parasemia plantaginis*, taken on the wing near the top of one of the hills on the same day. He also exhibited a specimen of the new British *Zygaena minos*, from the collection of Dr Fleming, in which it had stood probably for the last twenty years, and which Dr Fleming said he had no doubt had been taken by himself in Fifeshire.

4. *Notice of the Scops-Eared Owl (Scops Aldrovandi)*, Will. Orn. *Shot in Sutherlandshire.* By JOHN ALEX. SMITH, M.D.

This rare owl, which Dr Smith exhibited, was shot, in the latter end of last May, at Morrish, near Golspie. In the general colour and character of its plumage, it reminded him very much of the Nightjar; and is distinguished from our other British owls by its small size, by the incomplete character of its fascial disk, by its having tufts or horns, and also by its rather long and slender legs, closely covered with short mottled feathers, which terminate at the junction of the toes, leaving the toes entirely bare. There is also a series of spots along the edge of the scapulars, the outer half of these feathers being yellowish white with dark brown tips, contrasting beautifully with the closely mottled and minutely spotted and striped character of the rest of the plumage. It is a bird more especially of the southern and eastern portions of Europe, and from these it migrates to Africa. Several instances have been reported of its occurrence in England.

5. Mr A. Murray read an extract of a letter from Sir William Jardine, mentioning a capture of the Ivory Gull (*Larus eburneus*), shot at Thrumster, Caithness-shire. It was sent to him by Mr R. Shearer, Borrowston, near Wick, who has thus added another specimen to the two or three which are known to have been killed in Britain.

Wednesday, Dec. 27, 1854. Professor BALFOUR, P., in the Chair.

1. *On the occurrence of Oxalates in the Mineral Kingdom. Analyses of two new Species.* By M. FORSTER HEDDLE, M.D.

At this time last year two oxalates were known in the mineral kingdom. The one, an oxalate of iron, was analysed by Rammalesberg, and named by him Humboldtine; the other, an oxalate of lime, identical in composition with that ordinarily precipitated by the chemist, has been called after Dr Whewell. Some months ago Mr R. Greg of Norcliffe Hall sent me for analysis a few white crystals, which had been found, some five-and-twenty years ago, in a copper mine at the Old Man, near

Coniston Lake, in Westmoreland. From a hasty examination of these, Mr Greg was led to suppose that he had found a new substance, and the analytical result proved that he was right. I found the mineral to be an oxalate of lime, differing from Whewellite in having six additional atoms of water of crystallization. Associated with these white crystals was a purplish red substance, which, appearing to me to be new, I submitted also to analysis, when it proved to be an oxalate of potash, with ten atoms of water of crystallization. The colour was due to some oxalate of cobalt. It is always desirable that a mineralogist should be able to account for the occurrence of every substance which comes under his notice. This is more especially the case when the substance is of an organic nature, and in general we have little difficulty in satisfactorily explaining even such occurrences. The mineral Humboldtine, for instance, being found either embedded in lignite, or associated with decomposing succulent plants, leaves no room for doubting that, as it is organic in its matrix, so also it is organic in its origin. I am afraid, however, that our ingenuity will be taxed rather severely to account for the three other oxalates which we are now acquainted with, two of these having been found deep in the womb of earth, associated with a metallic lode. I think there can be little question that they are of secondary formation, having resulted in some way or other from the operations connected with the working of the mine; but I profess to be perfectly unable to offer any explanation which appears even to myself to be satisfactory. One theory has been brought forward,—a theory which I cannot but dissent from; it is, that the minerals were originally bi-carbonates,—that metallic potassium having been brought into contact with them, an atom of oxygen was abstracted, the result being necessarily oxalates. This does not appear satisfactory: neither bi-carbonate of lime or of potash have yet been found in nature; and I cannot place myself among those who, whenever they wish to account for volcanic action, or to get out of any difficulty, call in the aid of metallic potassium. I am very far from thinking that no satisfactory theory can be brought forward, but I am content for the present to look upon the occurrence of these oxalates as one of many proofs that as yet we know but too little of the operations carried on in nature's laboratory. The first of these minerals has been named by Mr Greg Conistonite, from the locality; and the second Heddlite, after the analyst.

2. *On a Raised Sea Bottom, near Filliside Bank, between Leith and Portobello.* By Hugh Miller, Esq.

3. *Exhibition of a Collection of Liasic Fossils from Pabba and Skye.* By Archibald Geikie, Esq.

Mr Geikie laid on the table the fossils he had collected, which he illustrated with the following remarks:—The Isle of Skye is an object of special interest to the geologist, from its containing in tolerable abundance the remains of the Liasic formation,—one which occurs in but unfrequent patches throughout the whole extent of Scotland. The Lias, as developed in that island, stretches from shore to shore in a band about seven or eight miles in length, by from two to five in breadth. Over the greater part of this extent a dark peaty soil covers the strata, so that they are seldom discernible, save where channelled by some mountain torrent. The best exposures are therefore to be found at the extremities of the belt. Broadford Bay, on the east, affords a general section of the formation. The beds are there free from the dislocating effects of trap dykes, and dip gently under the waters of the bay at an angle of 5°. The lowest members of the series are found at the village of Lussay, resting uncomfortably upon the red sandstone of Sleat. They consist of concretionary sandstones, and dark compact limestones, some of them charged with organic remains. But the most remarkable of these strata is one, irregularly three feet thick, composed entirely of corals of the family Astreidæ, which are bound together by an indurated mud. These organisms, of which there are several specimens upon the Society's table, were described several years ago by Mr Miller. They differ in size and abundance from any species in the lias of England, where corals are exceedingly rare; and they thus give a peculiar character and interest to the Scottish deposit. Beyond Lussay beds of sandstone and limestone alternate along the coast. Some of these abound with the characteristic shells of the period. At Breckish, for instance, where the limestone has been broken up in the course of constructing a road, the *Gryphæa incurva* might be removed from the beach by ship loads. The same fossil, mingled with ammonites, belemnites, and pectens, is found in most of the strata as far as Corrie Farm, at the northern point of Broadford Bay, where they are buried beneath an extensive overflow of Sienite. The upper members of the series are found forming the flat island of Pabba, about three miles out in the bay. Pabba, though not more than a square mile in extent, forms, with its rich green pasture, a striking contrast to the dark, barren mountains of the surrounding shores. The Lias is here represented by a series of dark micaceous shales, dipping northward at the angle usual in this district 5°. They abound with the organisms of the formation; indeed, so richly charged are some of the beds as to emit a strong fœtid odour when rubbed or broken,—a fact likewise noticeable in the Lias shales of Eathie. There is now on the table a set of these Pabba fossils. The majority have been already noticed by Murchison, and figured by Sowerby; but there are several which appear to be new. The most abundant organisms are the *Pectens*, of which there are at least three species. Other fossils are the *Pentacrinites*, *Plagiostoma*, and *Terebratula*, of each of which there are several species—*Gryphæa incurva*, and *G. Maccullochi; Pinna*, probably of several species; *Belemnites*, *Ammonites*, at least four species; *Serpulæ*, &c. The state of keeping of the fossils varies considerably in the different beds. The ammonites exist, in some cases, as mere flattened impressions. Generally they present only the outer ring, the central portion of the disc having entirely disappeared. In not a few of the layers the condition of the organic remains seems to indicate protracted maceration—a conclusion rendered probable by the abundance of casts of the more tender species. The western coast of Skye, along the shores of Loch Slapin, presents a rich field of study to the geologist. The Lias, for the space of several miles, is traversed in all directions by dykes and veins of basalt. In some places the limestone is black; in others, of different shades of gray; while inland, towards Kilchrist, it takes a snowy white; but in all cases it has been altered into a compact marble. A series of specimens upon the table exhibits the passage of a calcareous shale, abounding with Gryphæa and Pecten, into a hard fossiliferous limestone, which in turn shades off through various hues of black and grey into a white crystalline marble, destitute of organic remains. The latter rock, as it lies in the quarries at Kilchrist, is not much inferior in colour to the best stone of Italy, though, after being cut and exposed for a few years to the air, it acquires a dirty yellowish tinge. The trap dykes are themselves a curious subject for investigation. Owing to the decomposition of the marble around them, some of large size are seen running up the hill-sides like walls. Indeed, when two or three cross each other, the appearance presented reminds one of some ruined relic of the feudal times. Others may be found insinuating themselves among the cross rents of the contorted strata, and terminating in a point as fine as that of a pen. The shores of Loch Slapin are, on the whole, one of the most interesting localities in the island; and a careful examination of them would form a valuable contribution to Scottish geology. The district lies far out of the ordinary track of the tourist, and the accommodation, where it can be had, is not of the best: but these disadvantages would doubtless be more than compensated by a ramble among the beautiful sections which abound in the creeks and caves of that solitary shore.

4. *On some Worm Tracks in Silurian Slates.* By Alex. Bryson, Esq.

Mr Bryson showed that considerable difficulty was felt in accounting for these curious appearances on the Silurian slates at Thornielee, Peeblesshire. They had been named by Professor M'Coy *Crossopodia Scotica*, or fringed-footed animals. Sir Roderick Murchison described them as occurring of considerable length, even extending to yards. Mr Bryson was of opinion that the length was merely due to a track made by a worm of about six inches long, in mud of a rather crisp than slimy condition; and that the different appearances presented by the track, as compared with the surrounding matter, was due, not to the remains of the worm, but to dry dust blown into the track by the wind, on the recession of the ocean, which formed the lowest Silurian beds of Scotland. On the tracks found by Mr Bryson in the Llandeilo flags of Wales, he observed that many naturalists had mistaken for setæ merely the effects caused by wind blowing light sand over tracks made by gasteropodous molluscs; and stated, that tracks which he found at Port Rheudyn, in Wales, in almost the lowest beds of the Silurian slates, were quite identical with those he saw in the act of formation by the common Turbo littoreus, on the sands of Tremadock, a few miles south of Port Rheudyn. Mr Bryson exhibited some very large slabs, showing numbers of these tracks, sent him by the kindness of Mr Chaffers, the lessee of the quarry at Port Rheudyn, Wales.

January 24, 1855. Dr Lowe in the Chair.

1. *On the Discovery of Diatomaceæ in the Silurian Slates of Scotland.* By Alexander Bryson, Esq.

In a former paper, read at the last meeting of the Society, Mr Bryson had indicated a hope that Diatoms might be found in the lower Silurian formations of Scotland, from the peculiar appearance resembling organisms which he observed in a microscopic section of the slate from Thornielee Quarry, in Peeblesshire. One form is identical with a rare species found in the guano of Ichaboe, both in form and colour. In an endeavour to separate the alumina from the silica in the slate he had met with difficulties, as any solvent of alumina also acted on the silica of which he supposed the diatoms to consist. Dr George Wilson suggested the boiling of the powdered slate in Nordhausen sulphuric acid, which was found after a long time to isolate the silica. After many washings of the residue with distilled water, the author found several forms of diatomaceæ, two identical with living species, and four or five quite aberrant. After digestion with nitric acid the organisms seemed fewer, which he referred to their being more horny than silicious.

2. *Notes on a Species of Nostoc or Sky-Jelly* (specimen exhibited by Dr Heddle). By Alexander Bryson, Esq.

3. *Description of a New Species of Trematode Worm, with Observations on the Structure of Cercariæ.* By T. Spencer Cobbold, M.D.

Specimens of the worm were exhibited. They had been obtained from the liver of a giraffe, and differed from all known species. Dr Cobbold illustrated his paper with numerous drawings, showing the minute anatomy of this worm and also several embryonic forms of entozoa.

4. Mr P. A. DASSAUVILLE exhibited a specimen of the Gray Phalarope, (*Phalaropus lobatus*), Lath., which was shot in the Firth of Forth in December last. The bird was only beginning to assume its winter plumage, and appears to be a rare bird in this locality.

5. *Analysis of Datholite from Glen Farg.* By M. FORSTER HEDDLE, M.D.

Datholite, Dr Heddle said, has been found in the British islands in four localities, all of these being Scottish—first, by Mr Rose, on the yellow prehnite of Salisbury Crags; then at Glen Farg, in Perthshire, associated with zeolites, and well crystallized; next, upon prehnite, in what is mineralogically called the "Greenockite Hole," namely, the tunnel on the Glasgow and Greenock Railway; and, lastly, at Corstorphine Hill, by Mr Forrest, within the last few years. It is a fact worth notice that three out of these four are prehnite localities. This might warrant a searching examination for boracic acid in prehnite. In all these localities the mineral has been recognised by its crystallographic characters, no analysis of a British specimen having yet been published. A specimen from Glen Farg had been examined by Dr Heddle, and the analysis showed nothing different from those made of foreign specimens, with the exception of ·28 per cent. of oxide of iron; and as a second analysis (made upon crystals apparently absolutely pure) gave ·24 per cent. Dr Heddle was inclined to think that the iron is the colouring matter, giving the mineral its light yellowish-green or *asparagus stone* tint.

February 28, 1855. ROBERT CHAMBERS, Esq., P. in the Chair.

1. *On the late Severe Frost.* By HUGH MILLER, Esq.

Mr Miller remarked that the present intense frost,—coincident at new moon with a stream tide,—has killed many of the littoral shell-fish around our shores; and they now lie by thousands and tens of thousands along the beach. On the beach below Portobello, and for at least a mile on the western side of the town, they are chiefly of two species,—*Solen siliqua*, or the edible spout-fish or razor-fish, and *Mactra stultorum*, or the fool's cockle, both of them molluscs, which burrow in the sands above the low-water line of stream tides. The spout-fishes, when first thrown ashore, were carried away by pail and basketfuls by the poorer people; and yet of their shells enough remain in the space of half a mile to load several carts; but the fishes themselves, devoured by myriads of birds, chiefly gulls, have already disappeared. The Mactra, though they may be picked up in some places by basketfuls, are less abundant. It is probable, however, that both species will be less common on our coasts than heretofore, for years to come; and their wholesale destruction by a frost a few degrees more intense than is common in our climate, strikingly shows how simply, by slight changes of climate induced by physical causes, whole races of animals may become extinct. It exemplifies, too, how destruction may fall upon insulated species, while from some peculiarity of habitat, or some hardiness of constitution, their cogeners escape. There are two species of Solen in the Frith, *S. siliqua* and *S. ensis;* but we have not seen, on the present occasion, a single dead individual of the latter species; and, of at least four species of Mactra, *Mactra stultorum* seems alone to have suffered.

It is worthy of remark, that there are shells very abundant on the coast, and which, from their littoral character, must have been quite as much exposed to the intense cold as either *Mactra stultorum* or *Solen siliqua*, of which I did not find a single dead specimen on the beach. *Tellina solidula* is one of these species, and *Mactra solida*, with its sub-species or variety *Mactra truncata*, another; and these the frost seems not to have in the least affected. Of the various littoral univalves, too, including the periwinkles, purpura, and trochidæ, only one species—*Natica monilifera*—seems to have suffered. Now, *Tellina solidula* is in some localities,—as at Castleton King-Edward,—one of the most numerous and best developed of the boreal shells; *Mactra solida* is also a boreal species, with the common periwinkle *Littorina littorea*, the common purpura *P. lapillus*, and the dog-periwinkle *Trochus cinerarius*. Again, on the other hand, of the destroyed shells, I have not yet found any trace of *Tellina fabula* or *Donax anatinus* in the old glacial deposits, such as the boulder clay, or Gamrie gravels and sands, nor yet of *Mactra stultorum* or *Solen siliqua*, though the former is said to be a shell of the Mammiferous Crag, and the latter of the Clyde beds. And though a large *natica* occurs in both the Caithness and Gamrie deposits, that very considerably resembles *Natica monilifera*, it fails to exhibit the characteristic flexuous streaks, and in general form seems at least as much akin to a sub-arctic species as to the one recently killed by the frost. And there can be, I think, no doubt that the boulder clay Tellina, *T. proxima*, is altogether a different species, notwithstanding its points of similarity in the more dwarfish individuals, from *Tellina tenuis*. None of the molluscs killed in any considerable abundance by the present intense frost seem to be truly boreal species; and their destruction by the refrigerating agent, which has strewed them by millions along the beach, seems not only strikingly illustrative, as I have said, of one of the modes in which species may be destroyed, but also of a curious passage in the later geologic history of Northern Europe. It is an ascertained fact, that shells were living in the British area during the times of the Red Crag, of the same species with those recently killed by the frost; *Mactra stultorum* is one of these, and *Natica monilifera* another; and they now live in the neighbouring frith; but *I* at least have failed, after sedulous exploration, to detect them in the intermediate period of boreal shells, ice-grooved surfaces, and the boulder clay,—a period during which some of their hardier cogeners were very abundant. And the catastrophe which has just destroyed them in such numbers shows in part how this passage in our geologic history may have taken place.

2. *On the Silurian and Old Red Floras of Scotland.* By Hugh Miller, Esq.—Mr Miller illustrated his paper by the exhibition of a most interesting collection of the fossil remains of these little-known plants.

3. *On the Homology of the Vertebrate Skeleton, and its representative Eso-Skeleton of the Invertebrate Classes, with the application to Zoology, Palæontology, and Geology.* By Professor M'Donald.—The Professor exhibited a numerous collection of osteological preparations and diagrams in illustration of his peculiar views.

Botanical Society of Edinburgh.

9th November 1854.

1. *On the Associations of Colour, and Relations of Colour and Form in Plants.* By Professor Dickie, Belfast.

The author alluded to the harmony of colours in plants, and endeavoured to prove that the primary colours, red, yellow, and blue, are generally present in some parts of the plant; and that when a primary colour occurs in any part, its complement will usually be found in some other part. He also showed, that in regular corollas, the colour is uniformly distributed; whereas, in irregular corollas, there is an irregular distribution of colour.

2. *Records of new Localities for Plants.* By Dr Balfour.

3. *Remarks on the formation of Ascidia.* By Dr Balfour.

The author stated that he was induced to make some remarks on the formation of Ascidia in consequence of seeing lately a statement to the effect that all pitchers were formed by a hollowing out process. He was disposed to think that *true ascidia*, such as those of Nepenthes, Sarracenia, Cephalotus, and Heliamphora, were formed by folded leaves in the same way as carpels are supposed to be produced. The anomalous ascidiform productions on the leaves of cabbage, lettuce, &c., might be traced to a similar process, and in some instances the pitcher-like body appeared to be a second leaf folded in an opposite manner from that from which it sprung. Occasionally two or more leaves formed ascidia. What has been called the "hollowing out process" is applicable to such cases as Eschscholtzia, Myrtaceæ, Rose, Hovenia, &c. This hollowing out process caused a development of the circumference of the receptacle, peduncle, or other part, while the central portion was undeveloped, and thus there arose a cup-like body with a hollow centre. In such instances there seemed to be a union, in the early state, of the circumferential cellular papillæ arising from the peduncle or receptacle, or other part; these became elongated so as to form a gamophyllous rim of greater or less depth, enclosing a hollow space in which certain organs were developed. The pitcher-like peduncle or receptacle was often intimately connected with the calyx, and was lined by cellular matter in the form of a disk.

4. *On Linaria sepium of Allman.* By C. C. Babington, M.A.

The author stated that this plant had been found by Professor Allman, near Bandon, Cork, and that he (Mr B.) had been at first disposed to consider it and *L. italica* as hybrids between *L. vulgaris* and *L. repens.* Professor Allman had given conclusive evidence of the plant not being a hybrid; and from an examination of living specimens in the Cambridge Botanical Garden, Mr B. was disposed to look upon the plant as a distinct species, distinguished by its creeping root, erect smooth stems, linear-lanceolate acute scattered leaves, racemose flowers, ovate acute smooth sepals, shorter than the spur, and tuberculately-rough three-winged seeds.

5. *On Diseases in Plants caused by Mites.* By Mr Hardy, Penmanshiel.

6. *Botanical Notes.* By Dr J. D. Hooker, in a letter to Dr Balfour.

Dr Balfour stated, that in a letter recently received, Dr Hooker remarks (1.) that the natural order Balanophoraceæ is truly Dicotyledonous, and far removed from Rafflesiaceæ, the latter being (as Brown pointed out) closely allied to Aristolochias. The Balanophoraceæ are far more perfect in their ovules, and have albuminous seeds, with a Dicotyledonous embryo. They are closely allied to Gunnera.—(2.) Dr Hooker finds the germination of Nymphæaceæ to be genuinely Dicotyledonous. It is only the adventitious roots which are sheathed, as is the case with many other exogens. The rhizome of the order is a very reduced form of the exogenous, but not at all constructed on the endogenous type. The species of Nymphæaceæ must apparently be reduced to a very few, for in India half-a-dozen varieties in colour, number of petals, stamens and stigmatic rays, are found in one tank, and no two tanks have exactly the same forms. —(3.) Dr H. considers that Brown's theory of carpellary sutural placentation is the correct one, and that axile and free placentation may be reduced to it. Dr H. mentioned a case of Stachys with a four-lobed, one-celled ovary formed by two carpels placed back and front, and bearing half-way up a pair of parietal sutural ovules; also a Primrose with parietal ovules. The Yew which Schleiden describes as having an ovule terminating the axis, has been shown to have often two ovules, and when one, it is always oblique and lateral.

7. *On Stellaria umbrosa, Opitz.* By Mr G. Lawson.

Stellaria umbrosa, hitherto only known as a Sussex plant, had been observed by Mr Lawson on the shore, near Rosyth Castle, in Fifeshire. He did not, however, support its claims to specific distinction, and regarded it in the light of a book species, made out of forms of S. media; the Scotch S. umbrosa appeared to form even a greater departure from the typical S. media, than the Sussex one. Mr L. pointed out the characters which distinguished S. media, With., S. umbrosa, Opitz, (= S. grandiflora, Ten.), S. neglecta, Weihe, and S. (media?) microphylla, Wight; and exhibited specimens of all the forms in illustration of his remarks. No plant appeared to be more capable of adapting itself to all conditions of soil, climate, and situation, than Stellaria media, and to this circumstance was due the numerous forms of the plant known to botanists; the extremes of these forms were remarkably distinct from each other; but when studied in detail, all were found to be intimately linked together.

14th December 1854.

1. *Sketch of the Life of the late Professor Edward Forbes.* By Professor Balfour. This paper has been printed in the Annals of Natural History for January 1855.

2. *On Hypericum anglicum.* By Charles C. Babington, M.A., F.R.S. This paper has been printed in the Annals of Natural History for February 1855.

3. *On the Structure of the Anthers of Erica.* By John Lowe, Esq.

4. *Summary of the Flora of the Lake District.* By Mr James B. Davies.

11th January 1855.

1. *Notes on the Flora of Dumfries.* By W. Lauder Lindsay, M.D., Perth.

2. *Notice of plants in the neighbourhood of Oban, and in part of the island of Mull.* By David Philip Maclagan, Esq.

3. *On Plants found in Strachur, Argyleshire, and in Roxburghshire.* By William Nichol, Esq.

4. *On Lichens collected in the Breadalbane Mountains and Woods.* By Hugh MacMillan, Esq.

5. *On Harmonious Colouring in Plants.* By Professor M'Cosh, Belfast.

8th February 1855.

1. *Account of a Botanical Excursion to the Braemar Mountains in August* 1854. By Professor Balfour.

2. *Report on the Diatomaceæ collected in Braemar in the Autumn of* 1854, *by Professor Balfour and Mr George Lawson.* By Dr Greville. This paper appears in the Annals of Natural History for April 1855.

The following are among the novelties, some being new species and others additions to the list of British species:—

Eunotia Camelus, Ehr.
,, tridentula, Ehr.
,, quaternaria, Ehr.
Cymbella lunata, W. Sm.
Cymbella æqualis, W. Sm.
Navicula cocconeiformis, Greg.
Diatomella Balfouriana, Sm.
Orthosira spinosa, W. Sm.

3. *On the Geological Relations of some Rare Alpine plants.* By Dr Gilchrist, Montrose.

4. *Description of some new Coniferous trees recently introduced into this country by William Murray, Esq., of San Francisco.* By Andrew Murray, Esq. This paper appears in the present number of this Journal.

8th March 1855.

1. *A Comparative View of the more important Stages of Development of some of the Higher Cryptogamia and the Phanerogamia.* By Charles Jenner, Esq.—This paper will be found in the Annals of Natural History for April 1855.

2. *Notes of a Botanical Tour to the Island of Jersey.* By Mr C. Baxter, Royal Botanic Garden, Regent's Park. Communicated by Mr James Rae.

3. *On some Gall-like Appearances on the Leaves of a Species of Chrysophyllum from the Rio Negro*, collected by Mr Spruce. By Mr James Hardy.

4. *Extracts from a Letter from Dr Cleghorn on the Discovery, by Major Cotton, of the Gutta Percha Plant in Malabar.*—Communicated by Dr Balfour.—This notice will be found among the Extracts from Correspondence at p. 352 in the present number of the Journal.

5. *On some Plants which have recently flowered in the Royal Botanic Garden.* By Dr Balfour.—The plants referred to were Tricyrtis pilosa, Boucerosia Munbyana, and Erianthus japonicus.

Boucerosia Munbyana is noticed by Munby in his Flore d'Algerie, and the following are its characters:—Ramis tetragonis erectis, foliis ovatis acutis planis, floribus sessilibus, fasciculatis ad summitatem ramorum, laciniis linearibus, folliculis longissimis, apice inflexis. The plant has a habit of a Stapelia, is about five inches high, and sends off numerous branches, which are tetragonal and erect or ascending; the branches are more or less prominent, and have triangular concave depressions between them, and their edges are covered with triangular toothed projections, bearing minute, ovate, acute, fleshy, nearly sessile leaves. Flowers sessile, in clusters of 5-10 towards the extremity of the branches, of a brown colour, and fetid. Calyx five-partite, fleshy; segments narrow, acute, purplish-green. Corolla somewhat campanulate, five-partite, aestivation induplicato-valvate, and slightly twisted, segments rather more than a quarter of an inch in length, narrow, broader at the base, concave externally, the edges folding back during flowering, so as to give the segments a linear appearance, the internal lower surface showing numerous minute cellular papillæ. Staminal crown gamophyllous, consisting of five brown leaflets, each of which is trifid, the two lateral segments being erect or divaricate, and awlshaped, and the central portion triangular, acute, and incurved, so as to cover the anthers. Pollen masses yellow, elliptical, attached above the base where there is a sort of operculate margin. Stigma blunt.

The plant was sent to the garden by Mr Giles Munby. It was found by him on the rocks of Santa Cruz, and on the rocks overhanging the sea between Mers-el-Kebir and Cape Falcon, in Oran. The Arabs and the goats eat the young shoots.

Erianthus japonicus, according to Major Madden, occurs all along the Himalaya from Assam up to Simlah, growing on the northern sides of the mountains, in damp woods, and generally near rivulets, up to 7000 or perhaps 7500 feet, and is a fine species. It is noticed by Griffith, under the name of Saccharum rubrum, but it has no saccharine qualities.

6. *Observations of the Temperatures observed at the Royal Botanic Garden during the month of February last.* By Mr M'Nab.—The lowest temperature was 5° Fahr.

Californian Academy of Natural Sciences.

September 4, 1854.

Dr A. Kellogg in the Chair.

Dr Kellogg exhibited a drawing and specimens of a plant from the seashore and the salt marshes of the bay of San Francisco,—the *Frankenia grandiflora*.

Dr Ayres presented descriptions of the following species of fish, believed to be new:—

Zabrus pulcher, Ayres. This species is brought to the market from the 1st of August until the close of February, and is sold by the fishermen under the name of "*Black-fish.*" It is taken near San Diego.

D.12.10.; A.3.12.; P.18.; V.1.5.; C.14.

Hemitripteras marmoratus, Ayres. A species reaching from six to eight pounds weight. It appears to represent on this coast *H. acadianus* of the rocky shores of our Atlantic States; it is, however, entirely distinct from it, the structure of the head alone being enough to separate it.

D.11.17.; A.13.; P.14.; V.6.; C.10.

September 11, 1854.

Dr A. Kellogg in the Chair.

Dr Kellogg presented a drawing of a plant given him by Mr Wallace of Los Angelos, called by the Mexicans *Chia*. It belongs to the *Labiatæ*, but the genus is unknown. The seeds are said to be very mucilaginous, and are used medicinally in fevers and dysenteries, and other irritations of the bowels. It deserved the attention of the Academy.*

Dr H. Gibbons exhibited a head of bearded wheat, said to grow wild in the mountains. It measured about seven inches and a half in length. The grains are about half an inch long.

Dr W. Ayres continued his observations on the fishes brought to the market at San Francisco. *Rock-fish* or *rock-cod* is abundantly offered for sale. Five distinct species have been detected, although we were previously aware of the existence of only one, *Sebastes norvegicus*, Cuvier. Three of these are very nearly allied— *S. nebulosus, ruber*, and *parvus*, Ayres.

S. nebulosus, Ayres, is in colour finely mottled with dusky yellow and dark brown.

D.13.13.; A.3.8.; V.1.5.; P.7.10.; C.11.

S. paucispinis, Ayres; colour plain reddish brown above, lighter beneath.

D.13.13.; A.3.7; V.1.6.; P.5.; C.12.

PUBLICATIONS RECEIVED.

L'Institut, from 25th October 1854 to 28th February 1855.

Clark, William, History of the British Testaceous Marine Mollusca. 8vo. London, 1855. Van Voorst.

Proceedings of the Literary and Philosophical Society of Liverpool. 1853–54.

Quarterly Journal of Microscopical Science. Nos. 9 and 10.

Proceedings of the Californian Natural History Society.

Dublin Monthly Journal of Industrial Progress. January, February, and March 1855.

Les Cascades de Niagara et leur Marche Retrograde. Par E. Desor.

Westminster Review. January 1855.

Quarterly Journal of the Chemical Society. No. 28.

The Phonetic Journal. Vol. XIV. No. 1.

Journal of the Asiatic Society of Bengal. New series. Vol. XXIII. Nos. 69 and 70.

Davidson (Simpson), A New Theory of the Origin of Gold. 1854.

Journal of the Indian Archipelago and Eastern Asia. Vol. VIII. Nos. 5 and 6. May–June 1854.

Hooker, Journal of Botany and Kew Garden Miscellany. February and March 1855.

Journal of the Dublin Geological Society. Vol. IV. Part II. No. 2.

---

* Professor Balfour will feel much obliged by seeds of this plant and of the next being forwarded to the Edinburgh Botanic Garden.

SCIENTIFIC INTELLIGENCE.

ZOOLOGY.

Melanerpes formicivorus (Swainson.)—At a late meeting of the Royal Physical Society of Edinburgh, Mr Andrew Murray read a notice of a singular instinct possessed by a Californian woodpecker, which was said to lay up a store of provisions for winter use by boring holes in the bark of trees and placing in them acorns. (See Proceedings of the Royal Physical Society, *ante*, page 363.) A habit so singular and so little known among birds was listened to with some doubt, but on examining into the subject we find so many naturalists adverting to it that we cannot now refuse to give it credit. The following remarks on the habits of this woodpecker will be found in the very beautiful work on the Birds of California and Texas, by Mr John Cassin, now in the course of publication in America.

"Our present species (*M. formicivorus*) is one of the most abundant of the birds of California; it appears to take the place of the red-headed woodpecker in the countries west of the Rocky Mountains. Dr A. L. Heermann of Philadelphia made extended visits to California for the purpose of investigating its Natural History, and has identified, for the first time, this species of woodpecker, of which previously nothing could be accurately made out from the statements of travellers, and which was stated to possess the provident and curious instinct of storing away a supply of food for the winter in holes made for that purpose in the bark of trees.

"In the autumn this species is busily engaged in digging small holes in the bark of the pines and oaks, to receive acorns, one of which is placed in each hole, and is so tightly fitted or driven in that it is with difficulty extracted. Thus, the bark of a large pine, forty or fifty feet high, will present the appearance of being closely studded with brass nails, the heads only being visible. The acorns are thus stored in large quantities, and serve not only the woodpecker in the winter season, but are trespassed on by the jays, mice, and squirrels.

"The following intelligent account is from Kelly's Excursion to California:—'In stripping off the bark of this tree I observed it to be perforated with holes larger than those which a musket-ball would make, shaped with the most accurate precision, as if bored under the guidance of a rule and compass, and many of them filled most neatly with acorns. Earlier in the season I had remarked such holes in most of all the softer timbers, but imagining that they were caused by wood insects, I did not stop to examine or inquire; but now finding them studded with acorns firmly fixed in, which I knew could not have been driven there by the wind, I sought for an explanation. It is regarded as a sure omen that the snowy period is approaching when these birds commence stowing away their acorns, which otherwise might be covered by its fall. I frequently paused from my chopping to watch them in the neighbourhood, with the acorns in their bills, half clawing, half flying around the tree, and have admired the adroitness with which they tried it at different holes until they found one of its exact calibre; when inserting the pointed end, they tapped it home most artistically with the beak, and flew down for another.'

"But the natural instinct of this bird is even more remarkable in the choice of nuts, which are invariably found to be sound, whereas it is an utter impossibility in selecting them for roasting, to pick up a batch that will not have a large portion of them unfit for use. The most smooth and polished frequently contains a large grub. These woodpeckers never encroach on their packed stores until all the nuts on the surface of the ground are covered with snow, when they resort to those in the bark, and peck them of their contents without removing the shell from the hole. The bark of the pine tree, from its great thickness, and the ease of boring, is mostly sought for by these birds as their granary for the winter season."—(*Cassin—Birds of California and Texas.*)

Société Zoologique d'Acclimatation.—A new Society has been established in Paris under the above title. The objects of the Society are to encourage the *Introduction*, the *Acclimatation*, and the *Domestication* of useful and ornamental animals. The first meeting was held on 20th January 1854, under the presidency of M. Isidore Geoffroy Saint Hilaire. A report has been published, and the Society already numbers a long list of members.

Introduction of Foreign Species of Salmon.—At a meeting of "Academie des Sciences de Paris," 6th February 1854, Mon. Coste exhibited to the Academy specimens of salmon that had been hatched by him at the College of Frome. Similar results had been accomplished by Mons. de Vibraye in the fine establishment he had constructed on the banks of the Loire; by Mon. Desmé, at his demesne in the vicinity of Saumur; by Mon. Blanchet in the department of the Isere. The acclimatation of species at a distance from their native localities is not, therefore, so difficult as was supposed, and the following species have already been successfully introduced into certain waters of France:—The *Salmo hucho* of the Danube; *S. umbla; coregonus fera;* and into the Lake Ballon (Vosges) the great trout of the Swiss Lakes, *Salmo lemanis*, Cuvier.—(*Rev. and Mag. &c., Zool.*, 1854, p. 103.)

Eschara cervicornis.—This zoophyte has been discovered by Mr Embleton, in Embleton Bay, on the Northumberland coast. It was exhibited at the June (1853) meeting of the Berwickshire Naturalists' Club, by Dr Johnston, with the following remarks:—

"I have Mr Burke's authority for stating that our coral is the *Eschara cervicornis* of his catalogue of marine polyzoa. He is of opinion that it is identical with the *Cellipora cervicornis* of my British Zoophytes. The two specimens differ in habit, one being attached by a solid expanded base, the other by a cementation of the segments. The *Cellipora cervicornis* is, moreover, more erect in its mode of growth, and more solid in its texture; but these differences may be the result of age, and of peculiarities in the sites wherein the corals were developed. It would seem that although *Eschara cervicornis* has been often mentioned in works on the British Fauna, there are very few instances known of its occurrence on our coast. Dr Fleming has not included it in his 'History of British Animals,' so that the evidence for its being a native production must have been weak when that very valuable work was published. The species described in my 'British Zoophytes' was procured from the coast of Devonshire. Mr Burke did not know the exact habitat of his British species, for he seems to have seen only one. Thus Mr Embleton's is the third known British specimen, and it is the more valuable, as the locality is fully ascertained."—(*Proc. of Berwicksh. Nat. Club*, 1854.)

GEOLOGY.

Cause of the Gray Colour in Dolomite and other Neptunian Rocks.

Petzholdt has submitted to examination the opinion expressed by Göbel, that the colouring matter of dolomite depends on the presence of iron pyrites. He has examined seven different dolomites, and draws the conclusion that organic substances are the true cause of their colour. His experiments were made by digesting the specimens with hydrochloric acid, determining the carbon in the residue; and assuming that it exists there in the form of a humus acid, containing 58 per cent. of carbon, he calculates the total quantity of organic matter they contain. The following table gives his results:—

	Residue, insoluble in acid.	Carbon.	Organic matter.	Iron pyrites.
Dolomite from Tuttomäggi, .	14·90	0·102	0·176	0·35
Do. another specimen, . .	14·11	0·084	0·145	0·31
Dolomite from Igo Pank, . .	13·00	0·101	0·174	0·35
Do. do. Ojo Pank, . .	25·40	0·160	0·276	0·31
Do. do. Koggowa Sär, .	35·20	0·213	0·367	1·46
Dolomitic limestone from Hollenhagen, near Salzuflen,	26·61	0·131	0·226	none.
Dolomite from Ojo Pank, with black incrustation, . .	23·90	0·220	0·379	not exam.
Dolomite from Koggowa Sär, with black incrustation,	29·80	0·463	0·798	not exam.

By a comparison of these experiments with the colour of the specimens, the author draws the conclusion that it is due to the organic matter, and not to the pyrites.—(*Journal für Practische Chemie*, vol. lxiii., p. 193.)

[Our own observations have led us to a conclusion similar to that stated by Petzholdt. We have found that some dark-coloured limestones yield appreciable quantities of organic matter, amounting in some instances to about one per cent., without a trace of iron pyrites. In many instances, however, the organic matter is accompanied by pyrites, and this is remarkably seen in some varieties of black marble. The pyrites in such limestones may probably be traced to the collection of sulphate of iron during the decomposition of the organic remains which they contain.—*Edit. Phil. Journal.*]

CHEMISTRY.

Preparation and Properties of Aluminium. By M. St Clair Deville.

Some time since it was announced that Deville had succeeded in procuring aluminium in abundance, and by a process which would permit its use in the arts. It now appears that the processes employed by Deville are merely modifications of those already known, sodium and the galvanic battery being the agents employed to reduce the chloride of aluminium. These processes are manifestly so expensive as to render it unlikely that aluminium will be applied to any economic uses, but the author has been enabled to describe more fully than has before been done the properties of the metal. It is a fine white metal, with a high metallic lustre. Its hardness, when cast, is about the same as that of pure silver, but is increased by pressure. It is highly malleable and ductile, conducts electricity about eight times as well as iron, and is slightly magnetic. It crystallizes readily by fusion, and its crystals appear to belong to the regular system. It melts at a temperature above that of zinc, but lower than silver, and the author attributes the excessively high melting point found by Wöhler to the presence of platinum in the specimen examined by him. Its sp. gr. is 2·56, which is increased to 2·67 by rolling. It is unaltered by air and oxygen, even at the melting point of gold. It is without action in water, at ordinary temperatures, at 212°, and even at a lower heat;

but at a high temperature it slowly decomposes it. Nitric acid at common temperatures does not attack it and even when boiling, the action is excessively slow; nor is it soluble in diluted sulphuric acid. Its true solvent is hydrochloric acid, which attacks it very rapidly. At a very low temperature the gas attacks it, and converts it entirely into the chloride. Sulphuretted hydrogen is without action upon it. Aluminium does not amalgamate with mercury, but alloys with copper, silver, and iron. It gives a compound with carbon.—(*Annales de Chem. et de Physique*, vol. xliii., p. 1.)

Solubility of Carbonate of Soda.

Payen has made the observation that carbonate of soda, like the sulphate, has a point of maximum solubility. In fact the quantities of the crystallized carbonate dissolved at 57° Fahr., 97° and 219°, while the boiling points of the saturated solution are as follows:—

57°	60·4
97°	833·0
219°	445·0

It is remarkable that this peculiarity of so familiar a salt should have so long escaped the attention of chemists.—(*Annales de Chem. et de Physique*, vol. xliii., p. 233.)

On a Compound of Methyle and Tellurium. By Prof. Wohler.

This substance, which deports itself like a metal, is prepared by distilling a mixture of telluride of potassium and sulphomethylate of baryta. It is a reddish-yellow mobile fluid, heavier than water, and having an unpleasant alliaceous smell. It boils at 176°, and forms a yellow vapour. It burns with a blue flame, and thick fumes of telluric acid are formed. When boiled with nitric acid, nitric oxide is disengaged, and a nitrate of the oxide of telluromethyle is formed, which crystallizes in fine large striated prisms.

Oxide of Telluromethyle, $C_2H_3Te\ O$, is obtained as a white crystalline mass, without smell, but with a very disagreeable taste. It deliquesces in the air, absorbs carbonic acid exactly like caustic potash, and possesses powerfully alkaline properties, restoring the blue of reddened litmus and expelling ammonia from its salts. Sulphurous acid reduces it, separating the radical. It is obtained by decomposing the chloride or iodide with oxide of silver.

Sulphate of Telluromethyle, $C_2H_3Te\ O\ SO_3$ crystallizes in large transparent cubes.

Chloride of Telluromethyle is a white precipitate, very similar to chloride of lead. It dissolves in boiling water, and is deposited in small prisms. It melts at 207°·5, and is not volatile without decomposition. Treated with ammonia and oxychloride, $C_2H_3Te\ O+C_2H_3Te\ Ce$ is formed, which is also well crystallized.

Bromide of Telluromethyle resembles the chloride.

Iodide of Telluromethyle, $C_2H_3Te\ I$, is a beautiful yellow precipitate, which, some minutes after its formation, acquires a fine cinnabar red colour. When precipitated in a hot solution, it is obtained red and crystalline. It is very little soluble in cold water, more so in hot, and more abundantly still in alcohol, and deposits from these solutions in small red prisms. When its cold alcoholic solution is mixed with water, it is precipitated as a yellow powder, which, in the course of a few minutes, turns red. It is obvious, therefore, that this substance exists in two states, like iodide of mercury; but the author has not been able to ascertain whether this is accompanied by a dimorphous change. It is decomposed at 266°, producing the black iodide of tellurium.

A liquid sulphuret of tellurium appears to exist, but want of material prevented its examination.—(*Comptes Rendus*, 3d Jan. 1855.)

Examination of the Rind of the Mangosteen (Garcinia Mangostana). By Dr Schmid.

The rind was first boiled with water, which extracted tannin. The residue was then treated with hot alcohol; the filtered fluid deposited a resinous substance, which is a mixture of a resin and a crystalline matter, which the author calls mangostine. To separate it from the resin water is added to the hot alcoholic solution until it becomes muddy; on cooling the resin is deposited, and after standing for some the mangostine is deposited in silky plates, which are further purified by precipitating with basic acid of lead and decomposing the precipitate with sulphuretted hydrogen, and repeatedly crystallizing the product from alcohol.

Mangostine crystallizes in golden-yellow glittering plates without smell or taste, fusible about 374° into a yellow fluid, which solidifies into an amorphous mass; by farther heating it is decomposed, a small portion only subliming unchanged. It is insoluble in water, soluble in alcohol or ether, and the solutions are without action on litmus. It dissolves in the alkalies with a yellow or brown colour, nitric acid gives oxalic acid when boiled with it, and sulphuric acid dissolves it with a yellow colour, producing a partial decomposition. It is not precipitated by any of the metallic salts except subacetate of lead. Chloride of iron gives a dark blackish-green coloration. Its analysis gave results agreeing with the formula $C_{40}H_{22}O_{10}$. The lead compound was not obtained of constant composition. The author refers to the probable relations of this substance to the resin of gamboge, which has the formula $C_{40}H_{18}O_{21}$, and Purcee or Indian yellow, said to be produced from the urine of camels which have been fed on the fruits of Mangostana manganifer, of which the formula is $C_{40}H_{16}O_{21}$. He finds that euxanthic acid is a coupled compound decomposed by sulphuric acid into euxanthine and a substance which reduces oxide of copper.—(*Annalen der Chemie und Pharmacie*, vol. xciii. p. 83.)

BOTANY.

Gutta Percha of Singapore.—"Of the gutta percha very small quantities are now brought to Singapore; it has become a manufactured substance. A vast variety of its gum, at various prices, from three to thirty dollars a picul, is brought in by the natives. Some of these are deep red, some quite white, and many of them are hardly coherent, breaking down and crumbling between the fingers. These are cut and broken up, and cleared from the scraps of bark and wood which are generally found among them; they are then boiled in an iron pan with coco-nut oil, and stirred until thoroughly amalgamated. This mixture is allowed to cool again, when it is broken up and re-boiled with more oil, sometimes as often as four times, or until the mass acquires a certain tenacity. The good gutta percha, sliced into thin shavings, is then added in greater or less proportion, according to the quality of the basis, and the whole well mixed. The Chinese who do this are very skilful, and manage to produce from a great variety of gums a very uniform article,—wonderfully so, when it is considered that the gum is bought by the merchants in very small quantities at a time as the natives bring it in. . . . . There seems to be a great mystery about the Gutta Percha trees. I was in the heart of their country, and yet could get nobody to show me a single tree. I think the fact is, that they have all been long ago cut down within any reasonable distance of the settlements. I saw large quantities of the gum, though none of the best quality, on the Indragiri. I think I can distinguish at least five sorts, which are probably the produce of different trees; or rather five classes of gums, for perhaps the species are many more, and yet, though I offered great inducements, I could not get even a leaf. Of course, if I had gone up with time at my disposal, I would have seen the trees in spite of all; for I should have gone into the woods with the collectors, and this I hope some time to be able to do. The Gum Benjamin, another great staple here, I saw collected. The trees are about eighteen inches diameter, with small low buttresses to the roots; these are notched with a chopper, and produce the ordinary quality of the drug. The best, of a light buff colour and dense substance, is procured from wounds in the uncovered larger roots, and the common, or Foot Benjamin, is procured from the trunk of the tree. The oil of the seeds is valued as an application to boils; it is probably of little use."—(*Letter from James Motley, Esq., in Hooker's Journal of Botany*, February 1855.)

Mora excelsa, a large West Indian Timber tree.—" Prominent among the trees which adorn the forests of Guiana, and which astonish by their profuse verdure and gigantic size, stands the majestic *Mora*, the king of the forest. Rising to the height of from sixty to ninety feet before it gives out branches, it towers over the wall-like vegetation which skirts the banks of the rivers of Guiana, forming a crown of the most splendid foliage, overshadowing numerous minor trees and shrubs, and hung with Lianas in the form of festoons. The *Mora*, of all other trees of the forests of Guiana, is peculiarly adapted for naval architecture; and it is to be found in such abundance, that if once introduced for building material into the dockyards, there can never be any apprehension there would be a want of that timber which could not be supplied. The wood is uncommonly close-grained, and gives scarcely room for a nail when driven into it. When cleared of sap, it is durable in any situation, whether in or out of the water. With this property it unites another of equal consideration to builders; it is strong, tough, and not liable to split, has never been known to be subject to dry-rot, and is considered, therefore, by the most competent judges, to be superior to oak and African teak, and to vie in every respect with Indian teak. The full-grown tree will furnish logs from thirty to forty, or even to fifty feet in length, and from twelve to twenty-four inches square, taken from the main stem, whilst the remaining portions are suited to various purposes of naval architecture; such, for instance, as keels, keelsons, stern-posts, floors, ribs, beams, knees, breasts, backs, &c." Thus wrote Sir Robert Schomburgk fifteen years ago (*Transactions of the Linnæan Society*, vol. xviii. p. 207); and, in the same volume, that there might be no difficulty of distinguishing the tree in the search for it in other countries, Mr Bentham, from specimens sent by Sir Robert, published an excellent figure and botanical history, under the name of *Mora excelsa;* for it had previously no place in botanical works. It belongs to the natural order of *Leguminosæ*, and to the same group or section as the well-known *Cassias*. Yet it does not appear that the attention of any of our authorities or travellers has been directed to the commercial importance of this tree till very recently. The same tree has been found to prevail in certain localities of the island of Trinidad.—(*Hooker's Journal of Botany*, March 1855.)

Vegetable Oils in the Amazon and Rio Negro Districts.—Spruce remarks, that vegetables yielding oils abound in the Rio Negro district. Nearly all the palm-fruits yield oil; but the bright vermilion fruit of *Elais melanococca*, or Caiaué palm, furnishes it in very large quantity. Various species of *Œnocarpus*, which abound on the Amazon and Orinoco, are oil-bearing. The oil procured from *Œnocarpus Bataua*, which forms forests in the Rio Negro, is called Patana oil by the Indians, and resembles much that procured from olives. *Raphia tædigera*, the Jupati palm, has a very oleaginous fruit, and its leaf-stalks can be used as flambeaux. Andiroba-oil is the produce of *Carapa guianensis*. *Bertholletia* *excelsa*, the *Castanha* or *Juvia*, is another oil-giving tree of the Amazon district.—(*Hooker's Journal of Botany*, November 1854.)

Cyperus polystachyus.—This plant grows on the mouth of the crater of the extinct volcano of the island of Ischia. This is the only locality in Europe. It flourishes there where steam is continually issuing at a temperature of at least 150° Fahr. The plant is essentially a warm country species, tropical and extra-tropical in Asia, Africa, and America. In the European locality it is accompanied by *Pteris longifolia.*—(*Hooker's Journal*, November 1854)

Palma Jagua of the Orinoco.—Spruce mentions a species of *Maximiliana* having a frond 34 feet long, composed of 426 pinnæ, and a spadix, which bore about a thousand fruits, and was a load for two men. The palm was seen in the Orinoco, and is called *Palma Jagua.*

Fungus in a Cavity of the Lung.—A married woman, mother of four children, and 49 years of age, was in St Thomas's Hospital from the 29th November to 21st December 1853. She had been labouring for two or three years under the ordinary symptoms of chronic bronchitis, and of this disease she died. She was examined on 22d December, twenty-four hours after death. On examining the left lung there were found in its apex two communicating cavities, together about equal to a pigeon's egg. They were empty of secretion, but their parietes were moist, somewhat reticulated, and covered more or less by an opaque adherent film of fibrinous material. On the upper surface of the septum by which the cavities were imperfectly divided, was a soft velvety mass, occupying an area about equal to that of half a finger-nail, and measuring close upon a line or a line and a half in thickness. It was dry and powdery on the surface, and had a dull greenish hue. It was firmly attached to the wall of the cavity, and was clearly a mould or vegetable fungus growing in it. Under the microscope it exhibited a distinct mycelium, or a perfectly developed fructification. The mycelium consisted of delicate tubes, which terminated in nodulated roundish points, and varied between 1-8000th and 1-10,000th of an inch in diameter ; they branched in different directions, and presented here and there little bulgings, which doubtless were the commencement of new branches. The branches supporting the fructification were of considerable length, and much thicker than those belonging to the mycelium ; indeed, the largest measured about 1-2000th of an inch in thickness. They were cylindrical, without transverse septa, presented a well-defined limiting membrane, and pellucid structureless contents. At their free extremities they enlarged into globular or flask-like expansions, the greatest diameter of which was generally about twice that of the stalk from which they sprung. Their cavity was apparently perfectly continuous with that of the stalk, and their contents identical. The spores were situated on these expansions, and in the perfect heads these were so numerous and so thickly placed as almost to conceal them. The fungus clearly sprang from the walls of the cavity in the lungs, and most probably had grown during the life of the patient. The fungus resembles some of the Mycoderms figured by Robin, as occurring in the lungs of birds. It is probably much altered from its normal state, by the situation in which it grew.—(*Bristowe in Trans. of Patholog. Soc. of London*, vol. v. p. 38.)

Aloe-Wood, or Aloes of Scripture.—This fragrant wood appears to be produced by *Aquilaria Agallochum.* The tree is called in Hindi and Bengali Aggur, Agar, or Uggor ; it is also denominated Uûd, and the Arabic name is Aghaluji. Sanscrit writers give three varieties of Aloewood—1. Aguru, the common sort ; 2. Cálâguru or black aloes, being of a darker colour than the common kind ; 3. Maugalyá or Maugalyagura, having the fragrance of the Mallica or Jasminum Sambae. The name

Agallochum appears not to be derived from the Arabic, nor from the Hebrew Ahalim and Ahaloth, but from the Indian name Agaru, or with the Sanscrit pleonastic termination *ca*, Aguruca. It may be stated that the Portuguese Pao de Aquila, as noticed by Rumphius, is an undoubted corruption of the Arabic Aghâluji and the Latin Agallochum; and it is by a ludicrous mistake that from this corruption has grown the name Lignum Aquilæ, whence the genus of the plant now receives a botanic appellation, and which many authors have vainly attempted to distinguish from the Lignum-Aloes and Calambac. The generic and specific names of this plant are thus both drawn from the same original term.—(*Colebrooke in Lin. Trans.* xxi. 199.)

Origin of the Cultivated Wheat.—Much interest has been excited of late by the statements of M. Fabre and M. Dunal, who affirm that the cultivated wheat (*Triticum sativum*) is a variety of a grass called *Ægilops ovata* found in the south of Europe. This grass, under cultivation, is said to assume the form called *Ægilops triticoides*, and finally to become wheat. M. Fabre says that the complete change was produced in twelve years by constant cultivation. If this view is correct, then botanists are wrong in supposing wheat to be a *Triticum*, and it must be regarded merely as a *sport* of *Aegilops*, kept up entirely by the art of the agriculturist. We do not see common wheat in a wild state, but we meet with the grass whence it is derived. Wheat would seem to be a variety rendered permanent by cultivation. These opinions of Fabre have been supported by strong evidence. Of late, however, M. Godron has published a paper in the Annales des Sciences Naturelles in which he maintains that *Agilops triticoides* is not a mere *sport* of *Ae. ovata*, but that it is a hybrid between the cultivated wheat and the latter plant. This statement seems, at all events, to confirm the idea that wheat and the Ægilops are nearly allied plants, for hybrids are not easily produced except between plants which resemble each other closely. This would be the first known instance of a hybrid among grasses. There can be no doubt that the wheat and Ægilops ovata are congeners, and that they exhibit evident marks of resemblance in the form of their caryopsis. There appears, therefore, to be much plausibility in the statement of Fabre, and the hybridization spoken of by M. Godron may be merely such as would occur between varieties of the species. The matter is therefore by no means settled, and further experiments are required.

Balanophoraceæ.—Dr J. D. Hooker has examined thirty species of this order, and of twenty-six of these, both sexes. The simplest and most frequent form assumed by the rhizome or axis is that of a single or branched tuber, sessile on the root, whence it derives nourishment, and giving off one or more flowering peduncles. In the earliest stage of Helosidæ or Balanophoraceæ the plant appears as a cellular mass, nidulating in the bark of the root, (but partially exposed), with whose cellular tissue its own is in organic adhesion. At first there is no trace of vessels, but before it reaches the cambium layer of the bark the vascular tissue makes its appearance. Soon afterwards the wood of the root upon which the parasite grows appears to become affected; its annual layers are displaced, and at a later period vascular bundles enclosed in a cellular sheath appear to have ascended out of the axis of the rhizome, and to have become continuous with those already found in it. The rhizome sometimes attains great age. Helosis seems to be capable of indefinite increase. Phyllocoryne, as well as Rhopalocnemis, several species of Balanophora, Lepidophyton, Langsdorffia, and Sarcophyte are perennial. Cynomorium seems annual. The growth of the rhizome appears to be slow.

The vascular bundles in Helosis and Langsdorffia sufficiently show that Balanophoraceæ are Dicotyledonous. All the genera have an adherent perianth. In Cynomorium, the only genus with hermaphrodite flowers, the stamen is epigynous. Balanophors are epigynous calycíflorals, and ought, according to Hooker, to be placed between Halorageæ and Gunnera in a linear series.

A Balanophora growing on the Maple roots in Tibet produces great knots on the roots whence the Tibetans make cups. Cynomorium is an extra-tropical genus, attaining latitude 41° in Europe. Mystropetala and Sarcophyte inhabit South Africa. A species of Helosis is found in the La Plata district, and species of Balanophora and Rhopalocnemis in Northern India. Cynomorium coccineum ranges from the Canary Islands to the mouths of the Nile through 3000 miles of longitude. Rhopalocnemis is found in latitude 27° in East Nepal and Sikkim, on the Khasya mountains of East Bengal, and in Java near the line. Balanophora fungosa is found in East Australia and in Tanna, places separated by 1500 miles of ocean. Langsdorffia hypogæa is found in Oaxaca in Mexico, on the mountains of New Grenada, and at Rio Janeiro, a range of 4000 miles.—(*Jos. Hooker in Proc. of Lin. Soc.*, Feb. 1855).

Wellingtonia gigantea.—Dr Torrey has recently had an opportunity of counting the circles in a complete radius of the trunk of the famous Wellingtonia, now exhibited at New York, and he finds that they are 1120 in number. From the data furnished by Dr Torrey, we find that, on the radius examined—

	Inches.		Inches.
First 100 circles occupy a breadth of	17½	Seventh 100 circles occupy a breadth of	7¾
Second do. ...	14	Eighth do. ...	11
Third do. ...	12½	Ninth do. ...	10
Fourth do. ...	13	Tenth do. ...	11
Fifth do. ...	16¼	Eleventh do. ...	11¼
Sixth do. ...	8¾	The remaining 20 layers,	1

There are 1120 circles in a semi-diameter of 135 inches, or 11 ft. 3 inches. The facts show that the tree lacks about three centuries of being half as old as it was said to be. Its enormous size is owing rather to its continued rapid growth. Gray thinks that there is no adequate specific difference between Wellingtonia and Sesquoia, and that the tree must henceforth be called Sesquoia gigantea.—(*Silliman's American Journal*, vol. xviii. p. 286.)

Medicinal and Economical Plants of Victoria.—"The inestimable truth, that we may safely deduct the closest affinities of the medicinal properties of plants from their natural alliances—a truth which achieved the most complete triumph of the natural system over all artificial classifications—has generally guided me in tracing out which plants might be administered in medicine. By this guidance I observed, that our Pimeleæ are pervaded with that acridity for which the bark of Daphne Mezereum is employed; that our Polygala veronicea, the only described Australian species of a large genus, and in close relation to one lately discovered in the Chinese empire, not only agrees, like some kinds of Comesperma, with the Austrian Polygala amara, in those qualities for which that plant has been administered in consumption, but also participates in the medicinal virtue of Polygala Senega, from North America. Gratiola latifolia and Gratiola pubescens, Convolvulus erubescens, and the various kinds of Mentha, are not inferior to similar European species. The bark of Tasmania aromatica appears to me to possess the medicinal power of the Winter's bark, gathered from a similar tree in Tierra del Fuego; and its fruit is allied to that of the North American Magnoliæ used in cases of rheumatism and intermittent fever. The whole natural order of

Goodeniaceæ, with the exception perhaps of a few species, contains a tonic bitterness never recognised before, and discernible in many plants in so high a degree, that I was induced for this reason to bestow upon a new genus from the interior the name of Picrophyta. This property, which indicates a certain alliance to Gentianæ, deserves the more consideration, as the true Gentianæ are so sparingly distributed through Australia, while the Goodeniaceæ form everywhere here a prominent feature in the vegetation. Our Alps, however, enrich us also with a thick-rooted Gentian (G. Diemensis), certainly as valuable as the officinal Gentiana lutea; and in the spring, Sabæa ovata, Sabæa albidiflora, and Erythræa Australis, might also be collected on account of their bitterness. The bark of the Australian Sassafras tree (Atherospermum moschatum) has already obtained some celebrity as a substitute for tea; administered in a greater concentration, it is diaphoretic as well as diuretic, and has for this reason already been practically introduced into medicine by one of our eminent physicians. Isotoma axillaris surpasses all other indigenous Lobeliaceæ in its intense acridity, and can be therefore only cautiously employed instead of Lobelia inflata. The root of Malva Behriana scarcely differs from that of Althæa officinalis, and the Salep root might be collected from many Orchideæ. Few may be aware that the Cajeput oil of India is obtained from trees very similar to our common Melaleucas; and that even from the leaves of the Eucalypti an oil can be procured of equal utility. The Sandarac, exuding from the Callitris, or Pine-tree, the balsamic resin of the grass-trees, and, moreover, the Eucalyptus gum, which could be gathered in boundless quantities, and which for its astringent qualities might here at least supersede the use of kino or catechu, will probably at a future period form articles of export. Several Acacias are of essential service, either for their durable wood, or for the abundance of tannin in their bark, which has rendered them already useful, or for their gum; but the latter is even excelled in clearness and solubility by that obtained from Pittosporum acacioides. This species, as well as many other plants of the same order, is distinguished by a surprising, yet apparently harmless bitterness—a quality that warrants our expecting considerable medicinal power, and which deserves so much more attention, as till now we know nothing of the usefulness of the Pittosporeæ, although this order extends over a great part of the eastern hemisphere. The Australian manna consists in a saccharine secretion, condensed chiefly by the cicades from a few species of Eucalypti, but is chemically very differently constituted to the Ornus manna, and much less aperient. All our splendid Diosmeæ—a real ornament to the country—approach more or less in their medicinal effect to the South African Buchu-bushes. Baekea utilis, from Mount Aberdeen, might serve travellers in those desolate localities as tea; for the volatile oil of its leaves resembles greatly in taste and odour that of lemons, not without a pleasant, peculiar aroma. Trigonella suavissima proved valuable as an antiscorbutic spinach in Sir Thomas Mitchell's expedition; and the Tetragonella implexicoma, the various Cardamines, Nasturtium terrestre, or Lawrencia spicata, may likewise be used for the same purpose. The root of Scorzonera Lawrencii —a favourite food of the natives—would form, if enlarged by culture, an agreeable substitute for Scorzonera hispanica, or Asparagus; and Anistome glacialis—a large-rooted umbelliferous plant, from the snowy top of Mount Buller—will be added perhaps hereafter to the culinary vegetables of the colder climates. Seeds of the latter plants, amongst many others, have been procured for the Botanic Gardens. Santalum lanceolatum, Mesembryanthemum æquilaterale, Leptomeria pungens, and Leptomeria acerba, deserve notice for their agreeable fruit."—*(Report by Dr F. Müller, Government Botanist.)*

In a Report by Mr Swainson, an enumeration is given of 213 (?) species of Casuarina, commonly called He and She Oaks, but which, according to Mr Swainson, are the true pines of Australia.

MINERALOGY.

Mineralogy of the Dolomite of the Alps. By Sartorius von Walterhausen.

The author's investigations relate chiefly to the dolomite of the Binnenthal, which is remarkable from its containing in the middle of the deposit several small parallel veins containing a number of foreign minerals belonging to the classes of sulphurets, oxides, carbonates, silicates, and sulphates. Each of these groups have been separately examined.

Sulphurets.—These consist of zinc-blende in fine twin crystals, iron pyrites, small scales of orpiment, and beautiful transparent crystals of realgar. Besides these there is a gray sulphuret which consists of several minerals. This gray sulphuret was first described and analyzed under the name of Dufrenoysite by Damour, who ascribed to it the formula $2\text{Pb S} + \text{As S}_3$. It is described as crystallizing in the regular system, although its formula is identical with that of Federerz (glumosite) as analyzed by Rose, which contains antimony in place of arsenic, and whose form is prismatic. The conclusion to be drawn is, either that there exist dimorphous forms belonging to this composition, or that Damour's description refers to two substances, one examined chemically only, the other crystallographically. The author considers the latter to be true, having found by extended examination on the spot that several minerals exist which differ both in composition and form, but which are easily confounded in the massive state. For the mineral crystallizing in the regular system the name of Dufrenoysite is retained. The crystals generally have the form of the garnet dodecahedron or icositetrahedron. They occur isolated in the dolomite, and seldom reach the size of a pea. Their colour is dark steel-gray passing into iron-black. Their analysis gave these results, to which we have added those obtained by Damour for his mineral :—

		Damour.
Sulphur,	27·546	22·393
Arsenic,	30·059	20·778
Silver,	1·229	0·190
Lead,	2·749	25·999
Copper,	37·746	0·260
Iron,	0·824	0·380
	100·153	100·000

From which there can be no doubt the two substances are different. Von Walterhausen's result corresponds with the formule $R_2S + ASS_2 + RS$, and belongs to an entirely new group of minerals, being the first example of a sulphuret in which arsenic exists in the form of realgar. It is clear that Damour's analysis does not apply to the true Dufrenoysite crystallizing in the regular system; and, as an additional proof, the author has carefully determined the specific gravity of the regular crystals, and found it to be 4·477, while Damour found that of his mineral to be 5·549. On the examination of a large number of specimens from the Binnenthal lead, gray crystals belonging to the right prismatic system were found accompanying Dufrenoysite, which were so extraordinarily brittle that they could not be separated entire from the matrix. The ratio of the axes is $a : b : c = 1 : 0·96948 : 0·63385$, and several modifications were observed. Their specific gravity was 5·393, and analysis afforded the following result :—

Sulphur, ...	25·910
Arsenic, ...	28·556
Lead, ...	44·564
Silver, ...	0·424
Iron, ...	0·448
	99·902

This analysis does not lead to any simple formula, but the author infers that the mineral is a mixture of two substances having the formulæ $PbS + AsS_3$ and $2PbS + AsS_3$ in the proportion of about 3 to 1. The latter is obviously the mineral analyzed by Damour; and as the author has retained the name of Dufrenoysite to the mineral crystallizing in the regular system, he proposes to call this substance Scleroclase, while $PbS+AsS_3$, which, however, has not yet been obtained in the pure state, he calls Arsenomelan. The results of several other analyses show that these minerals may occur mixed in variable proportions although the crystalline form remains unchanged.

Oxygenized Minerals.—Among these occur magnetic iron ore, rutile, bitter spar, spathic iron ore, rock crystal, talc mica, and white and asparagus-green tourmaline. There are also two other substances of considerable interest. One is a fine right-prismatic crystal; hardness 3·5, specific gravity 3·977; it is a sulphate of baryta, containing 9 per cent. of sulphate of strontian. The other mineral is pure white, and sometimes perfectly transparent; hardness between felspar and quartz, specific gravity 2·805. Its crystals belong to the oblique prismatic system, and closely resemble those of adularia. The ratio of the axes is $a : b : c = 1· : 0·65765 : 0·54116$, and the inclination of z on $y = 64° 16' 8''$. Its analysis gave—

Silica, ...	24·127
Alumina, ...	49·929
Lime, ...	1·570
Magnesia, ...	0·420
Soda, ...	5·742
Baryta, ...	14·403
Sulphuric acid, . .	2·702
Water, ...	0·650
	99·543

From this he calculates the excessively complicated formula $5(3Al O_3 \, Si O_3) + 3(2RO \, Si O_3) = Ba OS O_3$, and gives it the name of Thyalophan. It is clear that this formula must at present be considered as very questionable.

The paper concludes with some theoretical considerations as to the mode in which dolomite has been produced.—(*Poggendorff's Annalen*, vol. xciv. p. 115).

Remarkable Brazilian Diamond.—The largest and finest diamond which has as yet been found in Brazil, has recently been imported into Paris, and has received the name of the "Star of the South." In its rough state it weighs 807·02 grains or 254½ carats. When cut it will be reduced to about 127 carats, and will therefore exceed the Koh-i-noor in size. Independently of its magnitude it possesses much scientific interest from the regularity of its crystalline forms, and the indications it affords of the mode in which the diamond occurs. The general form of the "Star of the South" is a rhomboidal dodecahedron, having each of its faces bevelled by a face set on very obliquely, so that it has in all 24 faces. On one of its faces there is a pretty deep cavity obviously produced by an octahedral crystal which has been implanted in it. The interior of this cavity when examined with a lens shows octahedral striæ, and it cannot therefore be doubted that the crystal which has left its trace was a diamond. On the posterior face of the crystal there are two other cavities of less depth also showing striæ, and one of them even exhibits traces of three or four different crystals. On the same side of the crystal there is a flat part where the cleavage appears, and which M. Dufrenoy considers to be a fracture, and possibly as the point by which the diamond was attached to its matrix. From these facts it appears that the "Star of the South" has been only one of a group of diamonds similar to the groups of rock crystal, calc spar, or any other crystalline mineral. The diamond is about to be cut, and will be shown at the French Exhibition, but it will then have lost its scientific interest.—(*Comptes Rendus*, vol. xl., p. 3.)

PHYSICAL GEOGRAPHY.

Relative Levels of the Red Sea and Mediterranean.—The French engineers, at the beginning of the present century, had come to the conclusion that the Red Sea was about thirty feet above the Mediterannean, but the observations of Mr Robert Stephenson, the English engineer, at Suez, of M. Negretti, the Austrian, at Tineh, near the ancient Pelusium, and the levellings of Messrs Talabat, Bourdaloue, and their assistants, between the two seas, have proved that the low-water mark of ordinary tides at Suez and Tineh is very nearly on the same levels, the difference being, that at Suez it is rather more than one inch lower.—(*Leonard Horner*, *Proc. Roy. Soc.*, 1855.)

An Uprise in the South Sea Islands.—Mr Royle, missionary at Aitutaki, in the South Sea Island, describes a dreadful hurricane which took place on that island on the 6th February 1854. He states "that the physical aspect of the lagoon, inside the distant reef of the island, is completely changed by the hurricane; so much so that he is inclined to suspect that some volcanic violence was at work. Some ten miles of new beach is raised up, composed of coral rock, sea shell, and rough sand, where before there was nothing but deep water."

MISCELLANEOUS.

Uncertainty of Preserving Records in Walls or Foundations of Buildings.—It is a common practice to place the coins of the time, newspapers, and other documents or records in sealed vessels, under the foundation stones, or in some marked situation in the walls of new public or otherwise important buildings. At a meeting of the American Philosophical Society in April last, Dr Boyé stated that, "On recently opening the corner stone of the present High School building of this city (Philadelphia), erected fifteen and a half years ago, in order to deposit its contents in the new building about to be erected, the papers, coins, &c. which had been deposited in a sealed glass jar were found to be in a perfectly decayed and corroded condition, and saturated with water. Dr Boyé stated, that after a careful examination he is satisfied that the water must have got in from the outside by infiltration, first through the mortar into the cavity, and afterwards from this through the sealing wax, with which the glass-stopper was secured. The corner-stone consisted of a block of blue marble, in which a rectangular excavation had been made, which was closed at the top by a marble slab sunk down into the stone and secured by common mortar. The lime used appears to have acted upon and corroded the sealing wax. The corrosion of the coins is ascribed to the sulphur in the glue or sizing in the paper.—(*Proceed. Amer. Phil. Soc.* v., p. 323-325.)

INDEX.

END OF VOLUME FIRST—NEW SERIES.

Neill & Co., Printers, Edinburgh.